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BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW
QUARTERLY ADVERTISER, JULY, 1854.

SURGEONS' HALL, EDINBURGH.

WINTER SESSION, 1854-55.

THE FOLLOWING COURSES OF LECTURES ON MEDICAL SCIENCE, and also those delivered in the University, qualify for Examination for the Diploma of the Royal College of Surgeons. All the Courses are for Six Months, if not otherwise specified.

Classes open on Friday, November 3.

Surgery, 10 A.M.—Mr. MACKENZIE.
Surgery, (4, High School Yards,) 10 A.M.—Mr. SPENCE.
Chemistry, 11 A.M.—Dr. GEORGE WILSON.
Natural Philosophy, (School of Arts), 12 NOON—GEORGE LEES, LL.D.
Clinical Medicine, (Royal Infirmary, Tuesdays and Fridays,) 12 NOON—Dr. WILLIAM ROBERTSON.
Clinical Surgery, (Royal Infirmary, Tuesdays and Fridays,) 12 NOON—Mr. MACKENZIE.
Anatomy, 1 P.M.—Mr. JOHN STRUTHERS.
Materia Medica and Dietetics, 2 P.M.—Dr. DOUGLAS MACLAGAN.
Practice of Physic, 3 P.M.—Dr. ALEXANDER WOOD.
Practice of Physic, (4, High School Yards,) 3 P.M.—Dr. W. T. GAIRDNER.
Analytical Chemistry, 9 A.M. till 4 P.M.—Dr. WILSON, assisted by Dr. MACADAM.
Practical Chemistry, (Three Months' Course,) 9 A.M. till 4 P.M.—Dr. WILSON, assisted by Dr. MACADAM.
Practical Anatomy, 9 A.M. till 4 P.M.—Mr. JOHN STRUTHERS, assisted by Dr. GREIG.
Anatomical Demonstrations, 4 P.M.—Mr. JOHN STRUTHERS.
Royal Infirmary, at NOON daily.

By Order of the Royal College, JOHN SCOTT, *Secretary.*

The INTRODUCTORY ADDRESS will be delivered by Mr. SPENCE, on THURSDAY, NOVEMBER 2, at 2 P.M.

FEES.—For the First of each of the above Courses, 3*l.* 5*s.*; for the Second, 2*l.* 4*s.*; Perpetual, 5*l.* 5*s.* To those who have already attended a First Course in Edinburgh, the Perpetual Fee for that Class is 2*l.* 4*s.* The Fees for the following Courses are,—Natural Philosophy, Practical Chemistry, and Practical Anatomy, 3*l.* 3*s.*; Anatomical Demonstrations, 2*l.* 2*s.*; Practical Anatomy with Demonstrations, 4*l.* 4*s.*; Analytical Chemistry, 2*l.* a month, or 10*l.* for the Winter Session. *Royal Infirmary,* Perpetual Ticket, 12*l.* 17*s.*; Annual, 5*l.* 7*s.* 6*d.*; Half-Yearly, 3*l.* 5*s.* 6*d.*; Quarterly, 1*l.* 13*s.*; Edinburgh Maternity Hospital, Six Months, 1*l.* 3*s.*

During the SUMMER SESSION, 1855, the following Three Months' Courses will be delivered:—

Natural Philosophy—Dr. LEES.
Medical Jurisprudence—Dr. RUTHERFORD HALDANE.
Midwifery—Dr. KEILLER.
Midwifery (4, High School Yards),—Dr. J. MATTHEWS DUNCAN.
Clinical Surgery—Mr. MACKENZIE.
Practical Anatomy, with Demonstrations—Mr. JOHN STRUTHERS, assisted by Dr. GREIG.
Practical and Analytical Chemistry—Dr. WILSON, assisted by Dr. MACADAM.

The above Courses of Lectures qualify for Examination at the Royal College of Surgeons of Edinburgh, England, and Ireland; the Apothecaries' Hall, London; the Faculty of Physicians and Surgeons of Glasgow; the Boards of the Army, Navy, and East India Company; and also, so far as required, for the Universities of London, Oxford, Cambridge, St. Andrews, Aberdeen, and the Queen's University, Ireland.

The Course of *Practical Anatomy,* if attended conjointly with the Royal Infirmary, qualifies for Graduation at the University of Edinburgh, and these two branches of the Curriculum of Study for the Edinburgh Medical Degree, whether taken alone, or with other prescribed Classes, constitute one of the Four Anni Medici required from Candidates.

Those who hold a Scotch or Irish Diploma, or Degree in Surgery, are admitted to the same rights, under the Poor Law Amendment Act, as Members of the Royal College of Surgeons of London.

Students are requested to bear in mind that the *Registration* at Surgeons' Hall closes on 30th November.

ST. GEORGE'S HOSPITAL

MEDICAL AND SURGICAL SCHOOL.

Session 1854—55.

Physicians—Dr. WILSON, Dr. NAIRNE, Dr. PAGE, and Dr. BENCE JONES, F.R.S.
Obstetric Physician—Dr. ROBERT LEE, F.R.S.
Assistant-Physicians—Dr. PITMAN and Dr. FULLER.
Surgeons—Mr. CÆSAR HAWKINS, Mr. CUTLER, Mr. TATUM, & Mr. H. CHARLES JOHNSON.
Assistant-Surgeons—Mr. PRESCOTT HEWETT and Mr. G. D. POLLOCK.

HOSPITAL PRACTICE.—The Hospital contains 350 Beds. Attendance of the Physicians and Surgeons daily, at One o'clock.

Surgical Operations on Thursdays, at One o'clock.

A MATERNITY DEPARTMENT, for the delivery of married lying-in women at their own homes, is established at the hospital, under the superintendence of the Obstetric Physician.

CLINICAL LECTURES are given during the Winter and Summer Sessions.

CLINICAL INSTRUCTION on the Diseases peculiar to Women is given in the Wards of the Hospital by the Obstetric Physician.

PRACTICAL PHARMACY, under the Superintendence of the Apothecary of the Hospital.

The Pupils attending the Medical Practice may become, when qualified, Clinical Clerks to the several Physicians.—The Pupils attending the Surgical Practice may become, when qualified, Clinical Clerks to the several Surgeons.

Pupils entering to the Surgical Practice for Twelve Months are allowed, when qualified, to dress the Patients for Three Months, and Perpetual Pupils for Six Months, without additional fee. The Dresser of the Surgeon of the Week boards at the Hospital free of expense.

The Perpetual Pupils are eligible to be Assistant House-Surgeon for Six Months, and House-Surgeon for Twelve Months (without additional fee), when properly qualified for the office.

THE WINTER COURSE OF INSTRUCTION WILL COMMENCE ON MONDAY, OCTOBER 2ND, WHEN THE SCHOLARSHIPS AND PRIZES FOR THE PAST YEAR WILL BE AWARDED.

Gentlemen becoming pupils of this Hospital may attend the Medical and Surgical Practice necessary for those who desire to become Members of the Royal College of Surgeons, or Licentiates of the Society of Apothecaries, and all the Lectures, on paying 40 Guineas at the commencement of the first year, 40 Guineas at the commencement of the second year, and 12 Guineas at the commencement of the third year. The payment for the year will admit the pupil to all the Lectures, and to the Hospital Practice required, and for that year only.

Special entries to Hospital Practice, or to any separate Course of Lectures, may be made as heretofore.

SCHOLARSHIPS, PRIZES, &c.

At the end of the Summer Session, Examinations will be held for Three Scholarships of the value respectively of 30*l*., 20*l*., and 10*l*., each tenable for two years; and at the end of each course of Lectures, Examinations take place of those students who are candidates for Prizes and honorary distinctions. Prizes are also given by Sir Benjamin Brodie and Dr. Chambers. The Thompson Medal will be awarded for the best Clinical Report of cases observed in the Hospital. Sir Charles Clarke's Prize for good conduct will be awarded to the student of the Hospital whose general good conduct most entitles him to this distinction.

Two Curators are appointed annually by the Weekly Board, on the recommendation of the Medical School Council, with a salary of 50*l*. each per annum. Two Registrars are appointed annually by the Weekly Board, on the recommendation of the Medical School Council, each with a salary of 20*l*. per annum.

An Obstetric Assistant is appointed by the Weekly Board, and resides in the Hospital.

Some of the Lecturers and other gentlemen connected with the Hospital receive students to reside with them.

Further information may be obtained from any of the Lecturers; or from the Apothecary of the Hospital, who is authorized to enter the names of Students.

CONTENTS OF No. XXVII.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1854.

Analytical and Critical Reviews.

Bibliographical Record.

Original Communications.

Chronicle of Medical Science.

Appendix.

THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

JULY, 1854.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *A Treatise on Diseases of the Heart.* By O'B. Bellingham, M.D., F.R.C.S.—*Dublin*, 1853. pp. 252.
2. *The Diseases of the Heart and the Aorta.* By William Stokes, Regius Professor of Physic in the University of Dublin.—*Dublin*, 1854. pp. 689.

Fifteen years ago, the publication of Dr. Williams' 'Lectures on the Diseases of the Chest,' and of Dr. Hope's work on the 'Diseases of the Heart,' induced a general belief that it was possible to make an accurate and minute diagnosis in almost every case of heart disease. Dr. Stokes contended against this belief at the time. The remarkable work of Dr. Skoda, while it shook this over-confidence in the powers of physical diagnosis, introduced, by a necessary reaction, a spirit of over-distrust.

Under these circumstances, the two works which head this article are valuable contributions towards a right knowledge of the diseases of the heart. Although both of them are upon the same subject, and emanate from the same city, so long distinguished for the eminence of its physicians, yet they treat the subject in a totally different manner. Dr. Bellingham begins at the beginning—describes systematically the heart in health and the heart in disease, brings together faithfully the later researches of various observers, and presents us with a very valuable compendium.

Dr. Stokes, on the other hand, plunges at once into the midst of his subject, takes us to the bedside of his patients, exhibits to us, with a vigorous pen, disease, not in a systematic manner, nor as we find it in systematic works, but as it is; every case being a fresh study, differing in origin, in features, in complications, and in the required treatment, from every other case. His work is, indeed, a series of original observations,

which come before us, like our own cases, in no very exact order, but fresh and full of instruction.

Dr. Bellingham's work is, in its very profession and nature, written for the present day: the record of what has been done by others, it will be succeeded by some future record of the labours of coming observers. Dr. Stokes' treatise, composed as it is of living pictures of disease, like his former volume on the diseases of the lungs, will last as one of the standards of experience, and take rank beside the works of Abercrombie, Cheyne, and Laennec.

A great part of Dr. Bellingham's work is occupied with a clear and detailed account of the healthy heart. The student of heart-disease cannot be too familiar with the heart in health, both in its actual and relative anatomy, and in its movements, its powers, and physical signs during life. Our author's descriptions are too condensed for an abstract, and we therefore strongly recommend the reader to study the work for himself.

Dr. Stokes commences with pericarditis. Previously to the appearance of Dr. Stokes' papers on Pericarditis in 1833, the diagnosis of that disease was most obscure. Collin and his friend Devilliers had noticed leather-creaking in isolated cases of pericarditis; and Broussais had observed parchment *frottement* in the early stages of the disease. With these exceptions, pointed out by Dr. Stokes, the only physical signs previously established were the increased extent of dulness on percussion, and marked prominence over the cardiac region. In the important paper referred to, our author established that, in pericarditis, a double *frottement* existed, often closely resembling a bellows or rasping murmur; disappearing gradually from below upwards with the increase of effusion, and returning with its decrease; and again disappearing from the apex to the base with the progressive formation of adhesions; limited usually to the region of the heart; changing in its character and seat from day to day; remarkably modified by local bleeding, passing from a loud *frottement* to a soft bellows-murmur; most rough and intense in some cases during inspiration; sometimes accompanied by a rubbing sensation communicated to the hand; and ceasing with the cessation of pericarditis. That paper—the value of which can scarcely be over-stated—found pericarditis the most difficult, and left it the most easy of detection of any of the diseases of the heart. The present work recapitulates, almost without modification, the points stated in the former paper—nay more the six cases given there are repeated here: this is well, for those cases cannot be too closely studied; but we own that we would gladly have had in their stead some fresh studies from the hand of the same master.

Dr. Stokes divides the cases of pericarditis into three forms:

"In the first are to be placed those in which there is but a slight, though general effusion of coagulable lymph. In the second we have superadded, the secretion of serum in abundance, causing distension of the sac. And in the third class we find, in addition to the preceding conditions, the signs of muscular excitement, if not of myocarditis.

"Let us contrast these forms.

FIRST FORM.	SECOND FORM.	THIRD FORM.
Absence of pain or local suffering frequent. No sign of muscular excitement, nor any special character of pulse. No increase of dulness over the heart.	The local and general symptoms more decided, though often very trifling. Irregular action of the heart and pulse, often more manifest in the advanced periods. Remarkable increase of dulness over the heart.	Local distress, often extreme, even at the outset. Tumultuous action of the heart. Irregularity of pulse. Dyspnœa, orthopnœa, œdematous swellings, syncope, death.

"These forms are not merely different in the degree of violence of the disease, but draw their distinctive characters from other circumstances. That there is a progressive increase in the violence of the original inflammation, as we ascend from the first to the third form, may be admitted. The great characteristic of the second form, however, is the effusion of fluid, while that of the third is the irritative or inflammatory excitement of the muscles of the heart. It is this which causes the great suffering, and, as we shall presently see, constitutes the danger in the advanced stages of the disease; for there can be little doubt that death occurs by syncope, induced by paralysis of the left ventricle, the result of its preceding excitement or inflammation. The muscles of the heart are then in the same condition as that of the intercostals after violent pleuritis; and when the weakened organ has not only to propel the column of blood, but to struggle with the pressure of a large body of fluid, while its action is clogged by a deposit of coagulable lymph, it is no wonder that it should fail to fulfil its function."

These three forms are, in reality, stages, or degrees of intensity, of the same disease. We doubt whether, in any case, coagulable lymph is effused, without, at the same time, increased effusion of serum. The amount of serum and of solid exudation are both proportioned to the severity of the attack. Dry pericarditis does not exist, we consider, as a variety; it does, however, as a stage. In the later stages the serum is removed, and the solid and dry exudation remains, and frequently glues the opposing surfaces of pericardium together. The paralysis induced by the inflammation of the muscular tissue is undoubtedly the most formidable effect of pericarditis. Rokitansky states that, under the influence of pericarditis, the muscular substance of the heart is paralysed, being of a dirty brown or yellow colour, flabby, and easily torn; a condition which speedily leads to passive dilatation of the heart, and general cachexia and dropsy.

"Two conditions of the muscles may be supposed to exist. One, simple atony or paralysis; the other a true myocarditis, attended with deposition of new matter among the fibres, or by ulcerative absorption. In the first of these conditions recovery is possible, just as we see in pleuritis that the action of the paralysed intercostals is restored, while in the second the organ appears to be irreparably injured.

"We may then conclude, that when death takes place as a consequence of pericarditis, the contractile power of the left ventricle at least has been seriously injured, and that the organ is either simply paralyzed, or that its structure has been altered more or less deeply by inflammation of the fibres themselves."

But, in many cases, death is not to be attributed to the cardiac inflammation alone, but to its complication with other diseases, general and local.

Dr. Stokes ranges acute rheumatism, gout, phlebitis, typhus, dropsy, and delirium tremens, as the chief general complications; pleuritis (generally of the left lung), pleuro-pneumonia, chronic empyema, chronic hypertrophy, fatty degeneration of the heart, and a group of typhoid

inflammations, as the principal complications with local diseases. Among these complications, Dr. Stokes does not specify Bright's disease, which existed in about 12 out of 40 cases detailed by Dr. Taylor, in the 'Lancet;' of these, 23 were complicated with acute rheumatism. Dr. T. K. Chambers states, that out of 135 fatal cases of pericarditis, occurring during ten years in St. George's Hospital, the probable direct causes were—

		Cases.
Rheumatic fever	in	18
Diseased kidneys	„	36
Diseased heart and dropsy	„	18
Pyæmia	„	18
Pneumonia	„	10
Vomicæ	„	8
Pleurisy	„	5
Other causes	„	22

Dr. Barclay* specifies the cause of pericarditis in 16 fatal cases:

		Cases.
Rheumatic fever	in	5
Diseased kidneys	„	8
Other causes	„	3

The first stage of pericarditis, before exudation, is not discoverable, Dr. Stokes says, by physical signs: this period rarely lasts longer than thirty-six hours. The question of the effect of the greatly increased vascularity of the inflamed surface, in causing friction sound, before exudation of lymph, is not handled by our author. His cases prove, however, that such is capable of exciting, or, at least, modifying the *frottement;* since he found that the application of a few leeches immediately modified and softened the morbid sound. Now, the application of leeches could not alter the exudation on the surface of the membrane, though it could and would materially lessen the turgescence of the minute vessels. The friction sound, therefore, in the cases referred to, must have been influenced by the inflammatory vascularity.

We need not follow our author in his description of the tactile signs, friction sounds, and extension of dulness over the heart.

The friction sounds differ over different parts of the heart, being stronger and rougher, especially during systole, over the right ventricle, to the left of the lower sternum, than over the right auricle, to the right of that bone; the two sounds being there nearly equally smooth and prolonged. In an established case of pericarditis, the exocardial can be distinguished from the endocardial noise by the nature and nearness of the exocardial sound—by its existence with diastole as well as systole—its limitation to the region of the heart—its non-existence over the great vessels—its variation over different parts of the heart—its rapid and frequent change in character, or its disappearance from day to day—its want of correspondence with the rhythm of the heart, while it seems to follow upon its movements (Skoda) or to precede and follow the impulse, or to come between the heart's sounds (Wünderlich), its co-existence with tactile vibration, and, where there is much effusion, with an extensive cone-shaped region of cardiac dulness—its apex pointing to the top of the sternum—its broad base extending downwards to the right and far to

* Medico-Chirurgical Transactions, vol. xxxv.

the left of the epigastrium:—if these signs, or a portion of them, co-exist, or are marked, the diagnosis of pericarditis is easy and certain; but in some cases of the disease throughout, and in the earlier stages of most cases, the exocardial so resemble the ordinary endocardial signs, that the most practised ear is occasionally deceived.

We have found the character laid down by Dr. Stokes, that the exocardial noises are limited to the region of the heart, usually borne out; but now and then a case occurs that offers a remarkable contradiction to this law. A case in which friction sounds have been exactly limited to the region of the heart suddenly presents a remarkable diffusion of harsh rubbing or grating noises over the front of the chest. We have in several cases found this diffusion of the friction sound to coincide with the disappearance of the fluid effusion; and we have noticed that the sound spreads in preference over the whole of the lower cartilages and ribs, from below the edges of the lungs. In these cases, it is manifest that the heart playing against the dry sternum produces a sound which consonates over the cartilages in connexion with that bone, where they lie, not over the lungs, but over the solid organs.

That the exocardial are often mistaken for endocardial murmurs is evidenced by the records of all the best observers. Thus, in only 8 of the 40 cases so accurately recorded by Dr. Taylor, was distinct friction sound heard from the first; while in 17 cases the reporter was doubtful whether a friction or a bellows sound was heard; and in 4, bellows sound, heard one day, was subsequently superseded by a rubbing noise. Dr. Stokes, Dr. Law, Mr. Mayne, and Dr. Graves, all present cases in which it was more or less doubtful whether a friction or a bellows-noise was heard. Weber, in his excellent manual on auscultation, recently translated by Dr Cockle, says that the murmur of pericarditis possesses no decidedly pathognomonic character.

Skoda, as rendered by Dr. Markham, thus expresses himself:—"According to my own experience, indeed, there is no kind of endocardial murmur, with the exception of the whistling, which may not be imitated by a friction sound of the pericardium; and no pericardial murmur which may not resemble an endocardial murmur." Skoda, perhaps, here goes too far; but there can be no doubt, that, although in most cases, especially in the advanced stages, the friction sounds are quite characteristic, yet we are in want of signs, in addition to those enumerated, to enable us to distinguish every case of pericarditis, especially in the earlier stages.

We believe that, in nearly every case, such a distinguishing sign is here afforded us:

"I have spoken of the effect of pressure. If, while the stethoscope is applied, we make a strong downward pressure with the hand, or increase the pressure of the head on the ear-piece, we shall often find a notable increase in the loudness and distinctness of the friction sounds; so that, in a case passing towards cure, we may reproduce, to a certain degree, the harshness and loudness which existed in the earlier periods of the attack. The same effect can be even better produced by causing an assistant to make pressure with the open hand on the cardiac region, during the application of the stethoscope. As might be expected, this modification by pressure varies directly as the elasticity of the chest. It is very remarkable in children, in women, and in young, feeble men.

"This mode of proceeding may be adopted in certain cases where we are in doubt as to the nature of the sounds. I have not made any extensive series of

observations on the effect of pressure in modifying the character of valvular murmurs, but it is certain that the pericardial sounds are much more influenced by pressure than those arising from valvular disease." (p. 19.)

It gives us deep satisfaction to find the valuable testimony of Dr. Stokes, in addition to that of Dr. Walshe, enrolled in favour of the aid afforded by pressure in the diagnosis of pericarditis; an important additional sign, which we thus brought before the profession, for the first time, in 1844:

"In the early stages of inflammation, where the old capillaries are enlarged, become spiral, and give existence to new vessels, and bulge up and roughen the pericardial surface, the ordinary friction of surface against surface is not sufficient to give birth to a rubbing sound. By pressing gently on the costal cartilage or sternum with the end of the stethoscope (I employ the flexible stethoscope), any fluid that may be interposed between the surface of the heart and the costal walls is now displaced. The opposed surfaces now touch; if they touched before, they are now pressed close together; and where the normal sounds were heard on light application of the instrument, or on applying the ear, a rubbing sound is now heard, due to the increased and now noisy friction of two turgid surfaces."*

"Very often a smooth friction sound, audible on applying the ear or the stethoscope lightly, becomes rough and harsh, or high and musical, on firm pressure. When the sound is audible on light pressure it is always increased in loudness, often in tone, by exciting firm pressure."†

By the aid of pressure, applied gently over the region of the heart, we have a test decisive as to the cause of the sound, when we are in doubt as to whether it be endocardial or exocardial. If the noise be a valve murmur, pressure does not increase or modify it, except in some anæmic persons, over the aorta; but if it be an attrition-murmur, but soft and bellows-like, pressure intensifies the noise, and converts it into a rustle or rub. This sign is of peculiar value in the stage of effusion, when pressure, by displacing the intermediate fluid, brings the roughened surfaces again in contact, and in the early stages, when it is in reality most important to arrive at a correct diagnosis,—since it will then often at once replace the prolonged rather rough systolic noise, noticed by Dr. Latham to be the frequent precursor of attrition-sound by a characteristic *frottement*.

Extensive effusion does not necessarily obliterate the friction sound. Dr. Stokes says:

"Again, as in pleuritis, the existence of a liquid effusion does not necessarily prevent the occurrence of friction signs, so in pericarditis does the same rule apply. . . . I have often found the friction sounds to remain at the base of the heart, long after extensive liquid effusion had taken place into the sac; and it is particularly necessary to insist on this, as it has been stated by some writers, that the third stage of pericarditis is not accompanied by *frottement*.

"In a case observed in the Meath Hospital some years ago, in which there was extensive dulness, the friction signs could be heard when the patient lay on his back, but disappeared on his assuming the erect position. The explanation of this is obvious. A case is given by Dr. Corrigan, in which the pericardium was enormously distended, so as to reach to the first rib. When the patient sat up, the friction sounds diminished, and sometimes altogether disappeared, but became well marked whenever he lay on his back. The heart was covered with a pulpy lymph, and there was a vast effusion of liquid into the sac." (pp. 19, 20.)

Dr. Walshe relates a case in which friction sound was heard at the mid-sternum, although sixty ounces of fluid were found in the pericardial sac, which reached a thumb's breadth above the clavicle. When exten-

* Provincial Medical Transactions, vol. xii. p. 540. † Ibid. p. 542.

sive pericardial effusion takes place, the heart is pushed upwards to the second, third, and fourth, intercostal spaces; consequently, the seat of the heart's impulse, of the rubbing sounds, and of tactile vibrations, are all correspondingly raised. This fact, which we noticed in 1844, is corroborated by Dr. Latham's 'Lectures,' and has been recently confirmed by Dr. Walshe.

"CASE I. *Simple Dry Pericarditis.*—Development of friction sounds and tactile vibrations. The sounds may at first be general or partial, and then spread over the whole surface of the heart. They may be at first soft, but rise to a maximum of roughness and loudness; when they commence to decline, becoming softer and more feeble. This change generally takes place first towards the apex, and extends to the base of the heart. They finally cease, the cardiac region remaining all the time with its natural sound on percussion.

"CASE II. *Pericarditis with Liquid Effusion.*—Friction signs are first developed with various degrees of intensity, but are generally less loud and rough in this case than in the preceding one. They soon disappear, either wholly or over a great extent, being still heard in some cases, principally at the base of the heart. The dulness diminishes, and with the return of clearness the friction signs reappear, though still generally feebler than in their first stage; then finally subside, leaving the sounds of the heart natural. The tactile signs may or may not be present at the commencement or resolution of the disease, but are seldom so well developed as in dry pericarditis.

"It is plain that in both these cases the diagnosis of an adhesion of the pericardium, more or less complete, can be easily made, not, however, from any direct signs of the condition itself, but from the fact of our having observed the exudation of lymph, with or without liquid, formed in a serous sac, and passing into organization. I more than doubt that there is any certain physical sign of adhesion of the pericardium, and have never been able to verify the sign relied on by Dr. Hope, of the double jogging impulse." (pp. 20, 21.)

Among the causes of modification of the friction signs, Dr. Stokes ranks the co-existence of air with the usual products of inflammation. He gives an interesting case, in which he inferred the presence of air in the pericardium. The rubbing sounds, though loud and distinct, had nothing unusual in their character, and the patient suffered but little distress. After two or three days his appearance was haggard and worn. The rubbing sounds, of the existence of which the patient was previously unconscious, had suddenly become so loud and singular, that the patient and his wife, who occupied the same apartment, were unable to obtain a moment's repose:

"On examination, a series of sounds was observable which I had never before met with. It is difficult or impossible to convey in words any idea of the extraordinary phenomena then presented. They were not the rasping sounds of indurated lymph, or the leather-creak of Collin, nor those proceeding from pericarditic with valvular murmur, but a mixture of the various attrition murmurs with a large crepitating and a gurgling sound, while to all these phenomena was added a distinct metallic character. In the whole of my experience I never met so extraordinary a combination of sounds. The stomach was not distended by air, and the lung and pleura were unaffected, but the region of the heart gave a tympanitic *bruit de pot fêlé* on percussion; and I could form no conclusion but that the pericardium contained air in addition to an effusion of serum and coagulable lymph.

"In the course of about three days the signs of effusion of air disappeared, leaving the phenomena as they were at the first period of the case. The convalescence of this patient was slow, and the rubbing sounds continued for an unusual length of time. His recovery was ultimately perfect." (p. 22.)

Dr. Graves, in his 'Clinical Medicine,' gives a case in which an hepatic abscess found its way, simultaneously, into the stomach and the pericardium. There was air in the pericardium, over which, on the day before death, a loud metallic tick, audible at each stroke of the heart, could be heard, combined with sounds having the character of an emphysematous crackling. Here the supply of air was manifestly from the stomach.

Dr. Stokes, in a foot-note, says:

"It may be placed in that important category of cases, which, independent of their rarity, may be taken as introductory to the diagnosis of new forms or combinations of diseases, or of affections previously known, but for the discerning of which no clear rules existed." (p. 24.)

Our author is mistaken in supposing that this important case is the first in which the signs of air in the pericardium have been recorded. M. Pigeaux, in his work on the 'Diseases of the Heart,' page 238 (1839), refers to two such cases previously published by him, in which there was "gargouillement," synchronous with the heart's beat, resembling the quick lapping of a dog when drinking. In one of the cases, the air entered and left at each stroke of the heart, producing a peculiar sibilant noise. M. Pigeaux—whose work, though sound and often original, is provokingly deficient in cases and references—does not state where these cases are published. Laennec, as our author states (page 28), considered that air in the pericardium might be diagnosed by unusual resonance on percussion over the lower sternum, by a noise of fluctuation caused by the beating of the heart, and by the heart's sounds being heard at a distance of from two inches to two feet from the patient. Andral, in his note on this passage, cites a case observed by himself, in which he heard a peculiar gurgling each time the heart beat against the ribs, and in which he inferred the existence of air in the pericardium; and another, recorded by Bricheteau, in which a noise like that of a mill-wheel was heard over the pericardial region with each beat of the heart. Percussion over the heart, after death, excited a "bruit de flot." The pericardium was filled with fœtid pus, and a certain quantity of gas.*

Dr. B. M'Dowel gives a case of pneumo-pericarditis, caused by its communication with an anfractuous cavity in the upper lobe of the right lung. The physical signs gave evidence of a large cavity, containing air and fluid, in the antero-inferior region of the left side of the chest; indicated by metallic tinkling (*bourdonnement amphorique*), and splashing of fluid, caused by the action of the heart.

"In Dr. Graves's case, the signs, though singularly modified, were still those of pericarditis; while in that by Dr. M'Dowel these were wanting; and a group of signs, closely resembling those of the ordinary empyema and pneumo-thorax, were produced." (p. 26.)

In a case of traumatic communication between the œsophagus and pericardium, referred to by Dr. Walshe, although no peculiar sound was heard, there was tympanitic resonance over the cardiac region, changing its seat with the change of position.

We possess the notes of the case of an old man who was run over by a

* Laennec, tom. iii. p. 394.

waggon, with the effect of rupturing the left upper lobe at its lower extremity. The air, which filled the whole of that side of the chest, had almost disappeared in a few days. "On applying the stethoscope lightly (over the heart), the sounds are heard dull, feeble, normal. On moderate pressure a very loud sharp click is heard during the impulse; this is preceded and followed by sounds resembling those caused by the successive movements of a spatula on a slab when triturating ointment." On examination after death, it was found that the small rupture in the lung was closed, but little air remained in the left pleural cavity, and the pericardium and adjoining pleura were remarkably dry.

Dr. Stokes warns us against being deceived into mistaking distension of the stomach with air for pneumo-pericarditis, when the friction signs of pericarditis may present a distinct metallic character:

"The last source of modification is the existence of valvular disease, either contemporaneous or previously existing. In certain cases this combination may cause some obscurity in diagnosis, but I believe that writers have over-estimated the amount of the difficulty. If we take the case of a previously existing valvular disease, the following circumstances will serve as means of diagnosis:—

"First. The actual acoustic character of the sound.

"Second. Its arising from a point comparatively deep-seated, and where it is at its maximum.

"Third. Its not being equably, or nearly equably, diffused over the surface of the heart.

"Fourth. Its greater extension over the thorax.

"Fifth. Its frequent want of the double character, the first or the second sound of the heart being often unattended with murmur.

"Sixth. Its being frequently transmitted along the aorta and its primary branches.

"Seventh. The absence of friction sensation communicated to the hand.

"On the last character it is to be observed, that the valvular tremor, like the sound, has, in many cases, a point of greatest intensity, and is not extensively diffused, as in pericarditis. Indeed, unless in some of the rare cases of varicose aneurism, the maximum point of the tremor is generally determinable without difficulty.

"There is, perhaps, a greater difficulty in settling the question when the disease affects the mitral valve, leaving the aortic orifice free; for in this case we have no transmission of the murmur along the vessels. A careful consideration, however, of all the phenomena will, in almost every case of doubt, lead us to a correct conclusion.

"I have already observed, that the signs of pericarditis must have often been mistaken for those of diseased valves. But their sudden supervention in a case where they had never before existed, the accompanying sign (when present) of the rubbing sensation communicated to the hand, the rapid change of situation, the equally rapid modification by treatment, and the occurrence of the signs with both sounds of the heart, in a case which previously presented no evidence of organic disease, form a combination of circumstances which can hardly mislead.

"But when it happens that, coincident with the attack of pericarditis, a diseased action is set up in the valves, the determination of the latter may be difficult, during the continuance of the true friction murmurs. If the valvular sign be, as it commonly is, a bellows-murmur, it may be completely masked by the loudness of the friction sounds, and only become manifest on their cessation. For some time, too, before these latter have wholly subsided, but when they have lost much of their loudness and roughness, it may be difficult to say how far the two sounds are intermingled. Yet the determination of the question is of importance only as relating to the prospects of the patient. It is a question of prognosis

rather than of treatment; and the case in question illustrates this important maxim, that in acute affections, when the diagnosis of the diseases of adjacent parts is difficult or impossible, it is often unnecessary, so far as treatment is concerned." (pp. 32—34.)

Perhaps the most important proof of the absence of mitral endocarditis is the existence of healthy heart sounds to the left of the apex—beyond the region of *frottement*. If, however, we hear a systollic bellows-murmur to the left of the apex, there is probably mitral endocarditis. We say probably, because the character of the bellows-murmur of endocarditis in no respect differs from that of established mitral disease. Indeed, if the patient has had a previous attack of acute rheumatism, attended by chest disturbance, followed by breathlessness or palpitation on going upstairs, the sound is most probably caused by pre-existing valvular disease. Under any circumstances, the presence of the mitral-murmur does not amount to a proof of endocarditis; for such murmur may unquestionably be induced by the mere disturbance of the muscular function of the heart, or by some other unexplained cause.

"It may be inquired, whether any assistance can be derived, in the diagnosis of pericarditis, from studying the acoustic signs which are proper to the muscular contraction of the heart, simply considered. This is a subject on which new researches are required, yet I cannot but think that some important results would follow from the investigation. It is to be determined whether any sign, independent of the irregularity of the heart's action, could be discovered, which would indicate the extension of disease to the muscular structure; whether the ringing sound of the ventricular contractions may be taken as a proof of the first stages of myocarditis; whether any purely muscular murmurs are developed; and lastly, whether, in the advanced stages of inflammation, the muscular sounds become weakened or destroyed.

"With reference to the last point I can state, that I have observed the disappearance of the first sound of the heart in cases of severe pericarditis; so that if we except the irregularity of action, the signs closely resembled those of the softened or weakened heart in typhus fever; and although the cause of this condition is pathologically different, yet, physically considered, it is the same in both diseases, and proceeds from the weakened state of the muscular fibres, resulting in one from the effects of inflammation, in the other from relaxation, with or without the interstitial typhoid deposit." (p. 35.)

Dr. Stokes finds, that in cases of dry pericarditis there is no alteration of the sound on percussion over the cardiac region. We have already stated that we doubt the existence of "dry" pericarditis, excepting as a stage of the disease. The healthy pericardium, we find, can hold, when distended, about six ounces in the boy, and about twelve or twenty ounces in the adult. It is not exactly ascertained what quantity of effusion must be present, before the lungs are pushed aside by it, so as to increase the cardiac dulness; perhaps the presence, in the boy, of between one and two ounces, and, in the adult, of between two and three ounces, would suffice. The increased dulness takes place upwards, over the great vessels, as Corvisart first pointed out. If the quantity of fluid be great, the fluid, by pushing aside the lungs, causes cardiac dulness not only upwards to the top of the sternum, or even up to or above the left clavicle, and sideways, sometimes even to the right nipple and to the left [illegible]l region of the chest, but it also pushes downwards the central tendon of the diaphragm from one to two inches, so as to displace the

stomach and liver, and to replace the gastric resonance by pericardial dulness.

Avenbrugger and Corvisart describe, with great accuracy, the depression of the diaphragm below the end of the sternum, caused by extensive pericardial effusion.* This effect of pericardial effusion is illustrated by our own diagrams in the 'Provincial Transactions' for 1844, and elsewhere; and by Piorry, in Plates 13, 15, and 18, in his 'Atlas de Plessimétrisme' (1851). Bock, in his 'Lehrbuch der Diagnostik' (1853), fig. 58, represents, with wonderful inaccuracy, the effects of pericardial effusion. In this figure, although the effusion is extensive, its lower boundary is actually an inch above, instead of one or two inches below, the lower end of the sternum. It by no means follows that the latest observers are the most accurate. Piorry's figures, 15 and 18, above referred to, show very clearly, but to an exaggerated degree, that change in position from the right side to the left, or *vice versâ*, causes the fluid in the pericardium to depend to the right or left, so as to alter considerably the right and left outlines of pericardial dulness. In figure 13, the outline of dulness is not sufficiently far to the right of the sternum. Dr. Stokes makes the valuable remark, that when the left side is dull in front, and resonant behind, it is a pericardial, and not a pleuritic effusion.

This large increase of fluid tells itself, especially in the young, by the protrusion of the left cartilages and ribs, the widening of their interspaces, and the prominence of the lower sternum; and in some extreme cases, as Sénac and Avenbrugger pointed out, by an epigastric tumour.

Gendrin† states, with unequivocal decision, that prominence over the epigastrium and lower part of the sternum exist in every case, and through every stage, of acute pericarditis, even when cardiac dulness is not increased, and the attack is of the slightest! This is anatomically impossible, and is contradicted by the evidence of the best observers. Thus, Rilliet and Barthez found that the thoracic prominence only existed when the extent of pericardial dulness was markedly increased, the one being proportioned to the other.‡

Perhaps the most important part of Dr. Stokes' chapters on pericarditis, is that relating to its vital symptoms and history.

The discovery of Laennec, while it illuminated disease in its local and physical changes and signs, turned away men's minds from the practical observation of the vital symptoms. The condition of the whole system was overlooked in the nice appreciation of minute visceral changes. While our view of disease gained accuracy, it lost breadth. The effect of this circumscribed diagnosis on treatment was disastrous. Your clever "stethoscopist," hearing a rustle, or perhaps a bellows-murmur, over the heart, at once subjected his unfortunate patient, though weak and free from actual distress, to a destructive course of bleeding, leeching, blistering, and mercurialization, with *diète absolu*. It is true, that he arrested the inflammation; but then he often arrested the heart also. What was lost in lives was gained in pathology. Under the eyes of Bouillaud, and of other physicians, post-mortem examinations revealed, with an amazing frequency, the existence of pericarditis; but then, they had had the signal satisfaction of predicting the disease during

* Sir John Forbes' translation, p. 57. † Maladies du Cœur, p. 469.
‡ Maladies des Enfans, tom. i. p. 625.

life. Their diagnosis was in every instance confirmed. The startling fact seemed to come out, that acute rheumatism, so long thought to be a disease only in the joints, was deadly in the heart. The older physicians, keen searchers many of them, had failed to detect this. There could be but one inference: acute rheumatism had suddenly assumed a new and a formidable type, and instead of limiting itself to the limbs, as in former days, it had spread to the heart also.

But the fact is, that it is not the tendency of rheumatic pericarditis to kill. It was too often the zeal of the physician that destroyed the patient. Meddlesome medicine is as mischievous as meddlesome surgery. A marked improvement in the knowledge and treatment of disease has prevailed of late years; and now the great discoveries of Laennec, taking their due proportions side by side with the other features of disease, are becoming daily of more real service to the physician and patient. We are indebted to the Dublin School of Medicine, more perhaps than any other, for this great improvement.

Dr. Stokes teaches that, in acute rheumatism, the pericarditis may precede the arthritis—that the liability to pericarditis is in direct proportion to the violence and duration of the fever—and that every variety and degree of pericarditis may occur in connexion with acute rheumatism, from the simple, dry, latent pericarditis, to the worst forms combined with inflammation of the endocardium and muscular structure.

"Although, as we might expect, the complication of acute rheumatism with pericarditis occurs under a variety of forms, yet three principal divisions of such cases may be made by the clinical observer. In the first, the disease, *as regards symptoms*, is truly latent, so that its discovery, which is only attainable by physical examination, is often accidental. In the second form, this latent disease may become manifest, and be indicated by a new train of symptoms, which at once draw attention to the internal disease, and it will then be found that the pericarditis has changed from the simple plastic form to a more severe affection, accompanied with copious effusion. This sudden change of dry, latent pericarditis into the more important forms of disease, is an accident which must always excite great alarm.

"In the last form the invasion of the pericarditis is attended by distinct symptoms of cardiac suffering, and these, as Dr. Mayne has shown, may exist for one or two days before the appearance of any tactile or acoustic sign of the disease. Of the local symptoms, pain and weight in the region of the heart, with an increased impulse of the organ, are not uncommon. The pulse may, in some cases, be wiry and regular, while in others, irregularity of the heart's action is one of the first symptoms. It is important to notice this, as we may commonly connect the idea of irregularity of the pulse with the weakened state of the organ in the advanced stages of the disease. Evidences of irritation of contiguous organs are often seen. The left pleura may present symptoms of disease, bronchitic or pneumonic râles may appear in the left lung, while vomiting and epigastric tenderness indicate that the stomach sympathizes with the diseased organ, or itself partakes in the irritation. In some cases the invasion of these symptoms is attended with a mitigation of the arthritis, but this is by no means usual. I have been more than once led to suspect pericarditis from a sudden increase of fever, without corresponding increase of tumefaction in the joints. The countenance is anxious, with a sense of sinking about the heart, and apprehension of death." (pp. 47, 48.)

The pain in pericarditis, when present, is frequently attended with a sense of constriction about the heart; it is seated over the sternum, and sometimes over the epigastrium and between the scapulæ. It is increased or excited by pressure, especially if made over the intercostal spaces, and

upwards, over the epigastrium. Sometimes it is agonizing; sometimes a mere uneasiness; frequently altogether absent. It resembles the pain of angina pectoris more than that of pleurisy.

The breathing is often difficult, high, and accelerated: while the movement of the upper chest is increased, that of the epigastrium is often arrested. This is a marked feature, especially when the central tendon of the diaphragm is involved.

In some cases, the patient stoops forward, so as to increase the space between the sternum and vertebræ.

The effects of pericarditis on breathing and on attitude depend chiefly on the amount of exudation. If the effusion be slight, the patient can generally lie upon the back, and the respiration is not usually distressed; if, however, the effusion is extensive, it compresses the trachea at its bifurcation backwards and upwards against the spine, so as to impede the entrance of air. Seven out of eleven cases given by Senac, of extensive pericardial effusion, were unable to lie down. In one, the patient sat with his legs bent. In a case detailed elsewhere, the patient sat all night in his chair, leaning on a table. When the patient lies down, the pressure upwards and backwards on the trachea is great; but if he stoop forward, the fluid gravitates upon the diaphragm, the space between the sternum and spine is widened, and the pressure upon the trachea is lessened. In a patient suffering from pericarditis with extensive effusion, we observed that the respirations were thirty-five in the minute when she lay down, twenty-nine when she sat up. The excessive dyspnœa, so often present in pericarditis with effusion, is not due to the compression of the lung—even extensive pleuritic effusion is often latent—but to the compression of the trachea, the inflammation and depression of the central tendon of the diaphragm, and perhaps to paralysis of the diaphragm itself, from compression of the phrenic nerves.

The pulse is very variable: it may be small and rapid, or singularly slow, or quite unaffected; it may be irregular, rapid, and feeble; while the action of the heart is sometimes feeble, sometimes violent.

Sometimes the face and lips are of a violet hue, but sometimes of a constricted pallor. The jugular veins are frequently distended. If the effusion be great, the distension is proportionate; if extreme, the venous distension does not lessen during inspiration. This last sign is to be observed in all cases where there is an absolute obstruction to the flow of blood through the chest; and whether the obstacle be in the heart or pericardium, the lungs or pleura, we believe it to be the most absolute and important sign of obstruction to thoracic circulation.

Œdema and great coldness of the lower extremities is noticed in cases of extreme pericardial distension, particularly in some of those given by Morgagni and Senac, Louis, Graves, and Mayne. This is readily explained by the compression of the thoracic aorta between the swollen sac and the vertebræ. In some cases, the left arm is swollen, probably from compression of the left subclavian at its origin.

The increased pulsation of the carotids, referred to by Dr. Stokes, is really a sign of accompanying aortic regurgitation.

By far the most formidable cases of pericarditis are those in which the functions of the nervous system are very seriously disturbed, although no organic change can be detected in the brain.

Jactitation, an extreme agitation, not permitting the patient to rest for a single instant in the same position, who, now sitting, now lying, in vain seeks for relief, is graphically described by Corvisart (see c. 14), in more than one case of pericarditis. M. Pigeaux truly remarks, that this jactitation is always a most alarming symptom.

Chorea in childhood, Dr. Walshe holds to be a most ominous complication: of 4 well-marked cases of the kind occurring in rheumatic pericarditis, 3 terminated in death. Dr. Kirkes gives 29 cases in which chorea was associated with heart-disease; of these, 16 had pericarditis, of which 9 died:* all had rheumatism but 2.

A tetanic affection was present in a young girl affected with carditis, whose case was related to Dr. Davis by Dr. Birkbeck. In Andral's case, (No. 8,) of pericarditis with delirium, there was, from time to time, tetanic rigidity of the arms, frequent retroversion of the head, and brusque movements of the body.

Among the same class of symptoms are the convulsive startings that suddenly rouse the sufferer when falling asleep, sudden starting out of bed, the twitching of the limbs and the convulsive distortion of features that present themselves to us in some of the worst cases of rheumatic pericarditis, and which are noticed by many authors.

Delirium, so often accompanying, alternating, or following the symptoms just described, has been admirably portrayed by our own observers —Dr. Davis, Dr. Latham, Dr. Watson, Dr. Bright, and Dr. Burrows.

Dr. Stokes has seen several cases complicated with delirium tremens, in which the pericarditis was but one of a group of irritations, all of them connected with that form of typhus which follows on an excessive debauch, and exposure to cold.

Testa gives some interesting cases of pericarditis with dysphagia. When we consider that the œsophagus lies between the pericardium and the bodies of the vertebræ, it is remarkable that dysphagia is so seldom noticed, for it must necessarily be exposed to the backward compression of the distended sac. Dysphagia existed in one of Dr. Watson's cases; and in one of Bouillaud's, epigastric pain was aggravated by swallowing. In a case of rheumatic pericarditis, related by Dr. Davis, the patient swallowed with difficulty on the day previous to death.† The same author (p. 133) states, that Daniel and Vogel both mention *hydrophobia* as a symptom of carditis. Gendrin‡ relates the symptoms observed by Trécourt in a remarkable epidemic of pericarditis, with copious purulent effusion, that occurred in the garrison of Rocroy, in 1746. The patients suffered from extraordinary thirst, but as soon as they desired to drink, they were attacked with symptoms of hydrophobia. Schönlein enumerates hydrophobia among the symptoms of serous carditis.§ This symptom is undoubtedly due to the distress excited by drinking. Dr. Walshe states that spasmodic dysphagia occasionally occurs in pericarditis.

In one case, given by Morgagni, the patient was obliged to swallow everything cold, owing to the distress excited by anything hot. In one of Louis' cases, the uneasiness and oppression increased after a meal, and lasted for two or three hours. Sir John Forbes states, that oppression of the chest after eating exists in almost all diseases of the heart

* Transactions of the Abernethian Society, vol. viii. † On Carditis, p. 71.
‡ Maladies du Cœur, p. 434. § Pathologie, p. 160.

and pericardium.* When we consider that the œsophagus is so immediately behind the pericardium, and between it and the spine, and that the stomach lies so immediately below the heart, it is remarkable that dysphagia and oppression after food are so rarely met with in cases of pericarditis, especially when there is extensive effusion. Dysphagia is not unfrequent in aortic aneurism, and Dr. Stokes has observed it in thoracic inflammation, when its accompanying phenomena seemed to prove that it was less the result of a mechanical condition, such as pressure on the œsophagus, than of some excited irritability, either of that tube, or of parts immediately in contact with it. We have analyzed the greater number of the published cases of pericarditis, and, with the above exceptions, we find no mention of the symptom in question.

"In a young man attacked with pericarditis, the voice underwent a great variety of changes of tone, and was not restored for several weeks. In this case, the liquid effusion was never very considerable. The phenomena were slight dulness, with various modifications of the rubbing sound." (p. 56.)

Dr. Davis gives a case in which there was hoarseness; and Wünderlich places aphonia and hoarseness among the general symptoms.

Dr. Stokes, in reasoning on the cases of dysphagia, says that it may be asked whether it may not proceed from inflammation of the retro-pharyngeal network of veins.

In connexion with the intimate anatomical relation of the œsophagus with the pericardium, we may mention two cases of great interest that we observed in St. Mary's Hospital. In one of these, which was under the care of Dr. Chambers, pericarditis was caused by an ulcerated opening from the œsophagus into the pericardial sac, through which the food found its way. In the other case, which was under our own care, and which is related in the Transactions of the Pathological Society, pericarditis was caused by extensive malignant disease of the œsophagus, where it lay behind the pericardium. Dr. Parkes relates the unique case of a juggler, who, when swallowing a blunt sword, thrust its point through the œsophagus into the pericardium, thereby causing pericarditis. In this case, Mr. Tidmas heard friction signs 40 minutes after the receipt of the injury. M. Pigeaux enumerates the arrest of a fragment of bone in the œsophagus among the causes of pericarditis, but, as usual, he does not refer us to any case.

We strongly recommend the close study of the cases of pericarditis related by Dr. Stokes. The seventh case is one of chronic empyema of the left pleura, with latent pericarditis affecting the displaced heart. The movements of the heart not unfrequently excite coinciding friction sounds, by the rubbing of the pleural surface of the pericardium on that of the ribs, in cases of pleuritis. It is very difficult in such cases to decide whether the pleuritis be associated with pericarditis or not. Usually, in such cases, the frottement is limited to the ventricles, and especially to the apex. There is seldom that equal to and fro sound over the right auricle which is heard in pure pericarditis.

Dr. Stokes makes some valuable remarks on the treatment of pericarditis, in which he warns us of the evils of a too energetic practice, especially as regards too free or repeated blood-lettings, a line of treatment

* On the Stethoscope, &c., p. 80.

which may weaken the muscles of the heart, as well as the system at large, and which is unnecessary as well as dangerous. In such cases, the boldness of treatment often betrays the timidity of the practitioner; he is terrified at discovering the disease, and his mind is more occupied with its name than its nature.

"It is my conviction that the fatal result of some cases of pericarditis is mainly attributable to the perseverance, beyond the proper time, in the antiphlegistic treatment; the practitioner looking at the disease merely as a case of serous inflammation, and forgetting not only the results of irritation on muscular fibre, but the effect of great losses of blood in producing reaction.

"Let us now suppose that we have a case of uncomplicated pericarditis in its earlier stages, and occurring in a patient whose strength is but little impaired: in such a case, a single bleeding from the arm appears, on the whole, justifiable, but its repetition will be a matter for careful consideration. Under these circumstances, we must examine the force of the heart, not only as indicated by the pulse at the wrist, but by the actual strength of the impulse, and the character of the first sound especially. If the impulse continues vigorous, and the first sound undiminished, we may be less apprehensive of the use of the lancet. On the other hand, if, after depletion, the impulse has manifestly declined in force, while the first sound is lessened, great caution must be used before we repeat the general bleeding. . . . .

"But our great reliance is to be placed on local bleeding, and the best mode appears to be the employment of leeches, in relays, beginning with twenty or thirty, and gradually reducing the number on each application. Two or three applications may be made in the twenty-four hours, a warm poultice being employed during the intervals. At the same time, it will be advisable to induce a mercurial action by such means as are within our reach, and it is probable that the plan of giving a full dose of calomel—say from ten to twenty grains—at long intervals, as recommended by Dr. Graves, will best answer our expectations. . . . .

"In the second stage of the disease our principal reliance must be on blisters; but we may apply leeches again and again on any new excitement of the heart. . . . .

"On the use of stimulants in pericarditis little or no information has been given by authors, yet they are often imperatively called for. I am convinced that cases are often lost from want of stimulation at the proper time. These considerations have pressed strongly on my mind since I made my observations on the state of the heart in typhus fever; and it is certain that in every case of dangerous pericarditis, after the first violence of the disease has been subdued, we should be anxiously on the watch for the moment when the weakened heart requires to be supported and invigorated. . . . .

"It may be laid down as a general principle, that there is no local inflammation whatever, the mere existence of which should prevent the use of wine, if circumstances require it. In two cases especially—namely, cerebritis and pericarditis—we find the greatest timidity in practice with respect to the use of wine. Yet, even in the first case it may be required, and in the second its employment is imperative, when, as too often happens, excessive depletion has been resorted to. Again, if the signs of muscular weakness, such as we have indicated, have appeared; if there be evidence that the heart, previous to the attack, was in a weakened state; and lastly, when a collapsed or typhoid condition of the system exists, we must give wine, quite irrespective of the physical condition of the heart. This may be done safely, and with great advantage. . . . .

"If we consider that extensive series of cases in which pericarditis occurs, either as secondary to a general or essential disease, or as one of a group of local inflammations, we shall find many cases in which wine may be used with liberality, even though endocarditis be present. Excluding the complication with ordinary rheumatic fever, we have to deal with pericarditis in connexion with the diffuse inflammations, or the low erysipelatous state; and again, in the pyogenic condition, as in the remarkable cases described by Dr. E. M'Dowel; in typhoid pneumonia;

and in the complication with delirium tremens from excess, already alluded to, which is so often attended with a typhus or typhoid fever. Many other cases might be specified, but enough has been said on the general question. There are two cases, however, sufficiently common to deserve notice here; one is the occurrence of the disease in the broken-down, gouty constitution, and the other that in which pericarditis attacks a heart in the earlier stages of fatty degeneration. Here the greatest faults in practice, both of commission and omission, are often seen; the original disease is unsuspected, and the patient held to have been in good health up to the time of the appearance of carditis, when the lancet on the one hand, and the debarring of stimulants on the other, at once reveal his condition, in most cases when it is too late to mend it. In truth, it may be said that no man is fit to treat general disease or local inflammation, especially its secondary forms, until he has conquered that fear of stimulants which a long course of erroneous teaching has instilled into his mind." (pp. 83—90.)

M. Merat, in the 'Dictionnaire des Sciences Médicales,' xl. 370, quotes two cases of pericardial effusion, in which M. Romero, of Barcelona, made an opening between the fifth and sixth cartilages, and then, by a pair of scissors, into the pericardium, so as to let a portion of the fluid run off; he plugged up the wound with a dossil of lint, by the removal of which daily, for two or three days, a further drain of the fluid took place. These two cases recovered.

We would strongly recommend paracentesis in all those cases in which the effusion is so great as to cause alarming distress, orthopnœa, obstruction to the venous circulation, and interference with the heart's action. In such cases we would employ the fine exploring trochar and canula, plunging it in below the heart, either to the left of the xyphoid cartilage, or through the fifth intercostal space, close to its anterior extremity. The fluid can be very easily drawn off through the finest canula by means of a syringe. This plan has been practised with great success by Dr. Bowditch, of Boston, in alarming cases of pleuritic effusion. There can be no necessity for attempting to remove a large quantity of the fluid; it will suffice if the tension be thoroughly relieved; this will be indicated by the disappearance of turgescence of the jugular veins. This turgescence is, no doubt, chiefly caused by the pressure of the effused fluid on the auricles, as was well remarked by Dr. Markham, and on the descending vena cava. M. Pigeaux suggests, that M. Recamier's fine exploring trochar and canula should be plunged in, an inch and a half to the left of the sternum, between the fourth and fifth cartilages (o. c, p. 209).

With regard to the treatment of rheumatic pericarditis, Dr. Stokes remarks—

"Whether rheumatic pericarditis demands any special modification of treatment is still an open question. The degree of activity of interference with the disease will, of course, depend not only on the character of the attack, but on the period of the fever in which it arises, and the strength and actual condition of the patient. In the two last forms it will be generally right to use mercury, pushed to salivation, not only with the view of controlling the pericarditis, but with the hope of preventing a chronic disease of the valves. Opium is generally useful, but I have never found that colchicum had any beneficial effect either in pericarditis or rheumatic arthritis, while the inflammatory fever continued."

Most of the above remarks by Dr. Stokes are of the highest practical value, especially those which relate to the judicious use of stimulants, and the true indications for their use.

But while we value this portion of Dr. Stokes' remarks on treatment, and that which applies to general blood-letting and colchicum, we own that we are surprised to find that his treatment is really heroic:—leeches in relays of from twenty to thirty, gradually reducing the number on each application—two or three applications in the day—and doses of calomel of from ten to twenty grains! Surely the exhaustion to the system caused by such a plan is a real and serious evil, not less deplorable than the problematical evil so much dreaded. Seeing the great weight that rightly attaches to any plan of treatment recommended by our author, we would protest all the more strongly against these ill-advised measures. We are the more surprised that Dr. Stokes should recommend this plan, since we find, at p. 47, the following passage:

"The treatment consisted in the exhibition of opium in large doses, as recommended by Dr. Corrigan, and succeeded admirably, none of the deleterious effects of the drug having been produced."

This relates to a patient treated by Dr. Graves, whose plan of leeching and calomelization is that so profusely enforced by our author. If we turn to Dr. Graves' own account of the case in question, we find that the opium, in doses of one grain every third hour, seemed to expend itself solely on the disease; for during the whole time she was taking it, it never produced contraction of the pupil, headache, hot skin, furred tongue, or constipation.* In this case, the mouth had been made sore, and a blister applied. In our own experience, we find that opium in such cases seems "to expend itself solely on the disease." The great value of this plan of treatment is, that the patient has a short convalescence, and, provided the endocardium escape, returns almost at once to the previous standard of health. In the early stages, it is advisable to apply a few leeches, and perhaps to conjoin calomel with the opium for one or two doses, treating, at the same time, the disease that lies at the root of the attack, on its own grounds. Pericarditis from acute rheumatism necessarily calls for a totally different line of treatment from that associated with Bright's disease, or diffused inflammation of a low type. One of the most important considerations in the treatment of pericarditis is the stage of the disease during which we detect it for the first time. It should never be forgotten that in pericarditis, as in pleuritis, the loudness of the friction sounds, and harshness of the tactile vibrations, are far from being in relation to the intensity of the disease—since, when the tide of the affection has turned, and the effusion is disappearing, the roughened surfaces, bearing the exuded but passive products of the earlier stages, come into closer and more diffused contact, and increase the extent and harshness of the rubbing noise, which then often bears an inverse ratio to the severity of the disease. If, under these circumstances, when the disease is of itself dying out, active measures be employed, the effect may be most disastrous.

Pericardial Adhesions.—Dr. Stokes remarks that—

"From our general knowledge of the history of serous inflammations, we must conclude that resolution without adhesion must be of very rare occurrence in pericarditis; and, consequently, it is fair to infer that, in most of the cured cases of the disease, an adhesion has really taken place."

We take exception to this inference, which is quite unsupported by

* Clinical Medicine, vol. ii. p. 181.

proofs, and is opposed to the general evidence. The very frequent occurrence of white patches on the surface of the heart, which are proved by Mr. Paget to be the result of pericarditis, shows that adhesions are not even the usual result of that disease. Mr. Paget discovered signs of old or recent pericarditis in 58 out of 110 bodies examined by him after death; in 4 only were there complete adhesions.* Dr. Kirkes' later observations confirm those of Mr. Paget.†

Dr. Stokes says, very justly—

"Without denying that a general adhesion may induce hypertrophy and dilatation, experience leads me to doubt that such an effect necessarily, or even commonly, follows the condition indicated. . . . . It is in those cases of pericarditis which we have before indicated, and when valvular disease is either co-existent with, or subsequent to, the first inflammation of the sac, that hypertrophy and dilatation appear as remote consequences of pericarditis. In the cases of recovery without murmur, we have little apprehension of the after-occurrence of organic disease. . . . . Where alteration of the muscular condition of the heart is found in connexion with this obliteration, it is not necessarily a state of hypertrophy, but is often one of an opposite nature." (pp. 11, 12.)

These remarks corroborate our own observations in the 'Provincial Med. Trans.,' for 1844. We there, after figuring three cases of adherent and greatly enlarged heart, cite four cases of loosely-adherent hearts, of normal size, that were free from valvular disease, and from any heart-disturbance during life. We also state, that—

"If the adhesions be dense, strong, and contracted, and unaccompanied by valvular disease, they often gradually lessen the bulk of the heart's cavities, and impede their expansion. A case of universal strong girdling pericardial adhesions, preventing the expansion of the cavities, the valves being healthy, and the heart weighing only $6\frac{1}{2}$ oz., presented the following symptoms: The heart's region of superficial dulness, impulse and sounds normal; pulse very feeble; palpitation, dyspnœa, and anasarca; lips blue."

Lancisi, Laubius and Garnerus, and Dr. Waugh, give each one case in which the pericardium was adherent and the heart small.

Dr. Chevers, in the seventh volume of the 'Guy's Hospital Reports,' gives four cases of firm pericardial adhesions, in which the heart was unusually small; in one of these there was also disease of the valves; at least two of the cases were in perfect health during life. Dr. Barlow, in his 'Gulstonian Lectures' for 1844, gives two cases of pericardial adhesions without marked valvular disease, in which there was hypertrophy and dilatation of the heart, especially of the right ventricle; and one case in which a ring of ossification surrounded the heart, and in which there was atrophy of the organ. Dr. W. T. Gairdner, Professor Smith, and other observers, have amply proved the truth of the above position as laid down by Dr. Stokes.

Senac and Corvisart considered that pericardial adhesions, unless partial or loose, prove very injurious to the action of the heart; while Laennec and Bertin thought adhesions usually by no means formidable.

When the friction sounds disappear gradually, from the apex to the base, as our author states, the diagnosis of an adhesion of the pericardium, more or less complete, can be easily made. He doubts, however, whether there is any certain physical sign of adhesion of the pericardium,

* Medico-Chirurgical Transactions, vol. xxiii.
† Transactions of the Abernethian Society, vol. iii.

and has never been able to verify the sign relied on by Dr. Hope, of the double-jogging impulse. Dr. Stokes also contends against M. Forget's view, that a general adhesion may be diagnosticated, when, after the subsidence of the friction sound of pericarditis, the heart assumes a permanently tumultuous and irregular action.

When the heart is large, and attached by the adherent pericardium, to the sternum and cartilages, the obstacle to its contraction is real and great. When to this condition is added valvular disease, or fatty degeneration, or resistance to the circulation through the capillaries, either of the system, as in Bright's disease, or of the lungs, as in emphysema, then the impediment to the circulation is truly formidable, especially if the whole of these causes co-operate.

Palpitation was present in 14 of the 55 cases of adherent pericardium included by Morgagni in his masterly analysis. In such cases, Burns noticed strong impulse extending to the epigastrium, which was accompanied, in a case of Dr. Rutherford's, by a jarring motion. This jarring sensation is also described by Dr. Ferriar, who found that the stroke seemed to be restrained, and was succeeded by a thrill, different from the shake of palpitation. Heim observed, during systole, retraction of the lower intercostal spaces, and of the space to the left of the xyphoid cartilage, followed during diastole by a shock; the retraction being greater and the shock less during inspiration. Sander describes similar retraction followed by a shock.

When general adhesions exist without enlargement, or with atrophy of the heart, it is impossible that any physical sign can indicate their presence. This cannot be said, however, of adhesions with enlargement of the heart, especially if the pericardium be adherent to the ribs and sternum. In such cases, we have observed a great extent of pericardial dulness on percussion, extending upwards to the second or third cartilage; great extent and strength of impulse; non-diminution of the extent of dulness on percussion, and of impulse on a deep inspiration; forcible retraction of the sternum during systole, followed by a shock during diastole; movement of the skin on the præcordia, during systole, from right to left; and lessened mobility of the lower left ribs and cartilages. Skoda, in corroborating most of these signs, adduces three cases in which there was systolic retraction of the intercostal spaces over the heart, an observation which had been previously made by Dr. Williams. We have observed this sign in cases in which post mortem examination revealed that there were no adhesions, but in these instances, the retraction ceased during a deep inspiration. The presence, however, of systolic retraction of the intercostal spaces over the heart, and its persistence during a deep inspiration, may be regarded as a physical sign of pericardial adhesion. Skoda found that the heart's apex gave no systolic beat in three cases, but he is wrong in generalizing this sign, since, whatever he may say to the contrary, the apex-beat was low, forcible, and systolic in two of our cases. In one of these cases, the pulse was intermittent during inspiration, and strong during expiration. The whole subject of the formation, frequency, effects, signs, and symptoms of pericardial adhesions is, however, imperfectly known, and demands a thorough investigation.

F. Sibson.

(To be continued.)

Review II.

1. *Maladies du Placenta.* By Murat. ('Dictionnaire des Sciences Médicales.' 1820.)
2. *Maladies du Placenta.* By Dance. ('Répertoire Général d'Anatomie.' Tome iii., 1827.)
3. *Maladies du Placenta.* By Brachet. ('Journal Général de Médecine.' Tome cii., 1828.)
4. *Maladies du Placenta.* By Cruveilhier. ('Anatomie Pathologique.' Livraisons i. et xvi., 1828.)
5. *Die Krankheiten des Mutterkuchens.* By Wilde. ('Medicinische Zeitung.' 1833.)
6. *Maladies du Placenta.* By Ollivier (d'Angers). ('Archives Générales de Médecine.' 1834.)
7. *Graphic Illustrations of Abortion and Diseases of Menstruation.* By Dr. A. B. Granville. 1834.
8. *Pathological Observations on the Diseases of the Placenta.* By J. Y. Simpson, M.D. ('Edinburgh Medical and Surgical Journal,' April, 1836.) Part. I.
9. *A Clinical Lecture on the Intra-Uterine Causes of Death.* By J. Y. Simpson, M.D. ('Edinburgh Monthly Journal of Medical Science, February, 1845.)
10. *Eine Krankheit des Mutterkuchens.* By Dr. Franz M. Kilian. ('Neue Zeitsch. für Geburtskunde.'. Vol. xxvii., 1850.)
11. *On Fatty Degeneration of the Placenta, and the Influence of this Disease in producing Abortion, death of the Fœtus, Hæmorrhage, and Premature Labour.* By Robert Barnes, M.D. ('Medico-Chirurgical Transactions.' 1851.)
12. *Specimen of Degenerated Placenta.* By Handfield Jones, M.D. ('Transactions of the Pathological Society of London.' 1852.)
13. *On Degeneration of the Placenta at the end of Pregnancy.* By Dr. Druitt. ('Medico-Chirurg. Transactions.' 1853.)
14. *A further Account of Fatty Degeneration of the Placenta.* By Robert Barnes, M.D. ('Medico-Chirurg. Transactions.' 1853.)
15. *On the so-called Fatty Degeneration of the Placenta.* By James Cowan, M.D. ('Edin. Med. and Surg. Journ.' April, 1854.)

The placenta, an organ located externally to the economy of the mother and of the fœtus, belonging exclusively to neither, and subserving but a temporary purpose, has been, until very recently, almost utterly neglected in the active and minute researches which modern pathologists have so successfully carried out to the elucidation of disease. And yet it may be safely anticipated, that the study of the physiology and pathology of this organ, connecting, as it does, the mother and the offspring, associated, as it is, with the earliest processes of nutrition and development, pre-

senting and elaborating, for the use of the embryo, the elements carried in the maternal blood, the medium for the elimination from the fœtal economy of its waste materials, cannot but be fruitful in splendid results. The great pathological as well as physiological agent is the blood. Hæmatology, taken in an extended sense, is a term almost synonymous with, and inclusive of, physiology and pathology. The changes in the constitution of the blood in health and disease, in their action and reaction, are at once the source of, and the clue to, all the variations in the character of the secretions and excretions, and all the alterations of structure to which the solid parts are liable. There is no organ, not perhaps excepting the spleen, the lungs, and the liver, in which such active and diversified processes of hæmatosis are carried on as in the placenta. In the adult, the spleen fulfils one office in the function of sanguification, the lungs another, the liver a third: the placenta, by itself, performs, or at least completes, the duties assigned to each of these organs. Harvey distinctly pointed out that the placenta performed for the embryo the function of the liver: "The liver, I say then, is the nutrient organ of the body in which it is found; the mamma is the same of the infant, and the placenta of the embryo." Lobstein has called the placenta a physiological lung. In this comparison he has but expressed a fragment of the truth. Goodsir expressed another portion of the truth when he attributed to the placenta the function of the intestinal canal. To complete the physiological history of the placenta, it must also be regarded as a spleen, as a kidney, and as skin. It may, indeed, be true, that the changes in the constitution of the blood essential to fit it for the purpose of nutrition, and the processes of purification and elimination rendered necessary by its contamination with effete matter, are partially wrought in the liver, the Wolffian bodies, the thymus gland, and other organs of the fœtus. The fœtal lungs are probably altogether passive; the liver is undoubtedly actively engaged in developing blood-corpuscles; the glandular structure of the skin is giving out an unctuous material, which is, however, largely mixed with epithelium scales;—this is probably a means of eliminating a certain proportion of carbon;—the intestinal canal* exhibits, at the period of birth, an accumulation of meconium, also a result of depuration; the Wolffian bodies, in early embryonic life, fulfil to a certain extent the function taken up by the kidneys at a later period; and the bladder, at the period of birth, usually contains a small quantity of urine.† Thus, that the blood, during its circulation in the fœtal vessels, undergoes numerous changes of a formative and depurative character, is certain.

But still we must look upon the placenta as the ultimate medium, the complementary agent through which all these processes are perfected. All the blood of the fœtus is successively brought by the umbilical arteries

* The opinion entertained by Harvey, that the embryo derives a *considerable* portion of its nourishment from the liquor amnii, can hardly be substantiated, although hairs derived from the lanugo, or first hairs of the fœtus, have been occasionally found in the stomach.

† Wöhler found *urea* in the liquor amnii in one instance. Dr. McClintock has established the existence of this substance in others; but numerous negative results prove that it is not a constant ingredient; and the source of it, when present—i. e., whether it be maternal or fœtal, is doubtful. According to Stass, urea may be extracted from the blood of the placenta. The urine of young children scarcely contains a trace.

to the placenta, where it exchanges, with the maternal blood, its waste materials for fresh alimentary matter. The placenta is the final emunctory channel, and the prime restorative source. It may be comprehensively stated, that the placenta is the organ in which all the functions of ingestion and egestion of the fœtus are performed. Where is the other organ that is so vascular, so overflowing with blood? The placenta is literally a mass of bloodvessels. It undoubtedly contains a greater proportion of blood than any organ of the adult body. Where there are so many blood-vessels, so much blood, such activity and diversity of function, there must surely be a corresponding liability to disease. Further, reflection must convince every one that the physiological and pathological conditions of the placenta must exercise the most important effects upon the economy of the mother on the one hand, and on that of the fœtus on the other. The investigation of these conditions of the placenta cannot but reflect a brilliant light in both these directions, maternal and fœtal. Placed between the mother and the fœtus; forming, for the time, a part of each, in which reciprocal changes are being constantly effected, in which the properties of the circulating fluid of each organism are transmitted to the other, can we conceive it possible that a perfect comprehension of the pathology, either of the pregnant woman or of the fœtus, can be obtained, if we overlook the intermediate link?

We will state an illustration. The frequent presence of albumen in the urine of pregnant women is a remarkable fact. It has been attempted to explain it in various ways, but mostly without reference to any other condition than that of the mother individually. Dr. Lever, who was the first to point out the fact of the co-existence of puerperal convulsions and albuminuria, still adheres to the opinion that the presence of albumen in the urine of pregnant women is to be accounted for on the principle of mechanical obstruction to the function of the kidneys, created by pressure of the gravid uterus upon the kidneys and emulgent vessels. And yet it is certain that this pheuomenon is occasionally observed at periods of pregnancy, and under circumstances which do not admit of this explanation. The real cause must be sought elsewhere. The albumen found in the urine is the result, and an indication, of the degradation of the mother's blood, wrought, chiefly at least, by the abstraction of its healthy elements in the placenta, and the reception of the effete materials of the fœtus, discharged into it through the same organ. It frequently, indeed, occurs also, that the womb, enlarging, encroaches on the space required for the perfect play of the abdominal and thoracic organs; and that an increasing indisposition for exercise augments the difficulty of ridding the blood of its impurities by means of the excretory glands, and of replenishing it by the assimilation of new materials; and that there are thus superadded other causes of deterioration. It can scarcely, however, be disputed, that the first origin of the contamination lies in the embryo and its appendices.

Let us point to another illustration. What do we know of the pathology of the fœtus in utero? In how many instances is the fœtus cut off at various epochs of intra-uterine existence? How seldom are we able to trace the problem of its death to a satisfactory solution! The most diligent dissection of the fœtus leads to no result. The cause is sought

where only the effect is visible. Functions all-essential to the development and life of the fœtus are performed externally to its organization.

It is in the seat of these functions,—in the placenta,—that the cause of their interruption or arrest, and the consequent destruction of the fœtus, must be looked for. The integrity of the placental structure, or at least of so large a portion of it as is required to perfect the necessary reciprocal changes between the maternal and fœtal blood, is of more vital importance to the fœtus than perhaps is that of any internal organ. A somewhat extensive, and certainly careful observation, justifies us in affirming, that the fœtus perishes less frequently in consequence of any defect or disease commencing in its permanent organs, than it does in consequence of an improper condition of the maternal blood, or of disease obstructing the placenta in the performance of its functions. The evil effects, therefore, of disease of the placenta radiate in two opposite directions: they influence the mother, and may be fatal to the embryo.

In the adult, certain pathological conditions of the body are traced distinctly to obstruction or disease in the lungs, heart, liver, and kidneys. Congestions, various effusions, especially dropsy and serous inflammations, are amongst the more remarkable of these. All these conditions are frequently observed in the fœtus. What is their source? Does it lie in the lungs, heart, liver, or kidneys of the fœtus—organs which only partially perform the functions they are, at a future time, destined to fulfil—or in the placenta, which is as yet the chief agent for their accomplishment?

There are three relations of the affections of the placenta which may be studied with advantage:

1. There are morbid conditions of the placenta which may originate in its own structure, just as inflammation may develope itself in a particular organ of the body.

2. The morbid conditions may result from the state of the blood of the mother brought into it, or from contact with diseased uterine structures.

Both of these two classes of alterations imperil the well-being of the fœtus, and may be the primary causes of its destruction.

3. The morbid conditions may be secondary to disease, or defective developmental force, in the embryo itself.

In this last case the changes in the placenta are the effect and not the cause of the decline, or death of the fœtus.

In any given case, the task of unravelling the complicated pathological relations, whether of causation or of consequence, associated with the morbid alterations observed in the placenta, is a matter of difficulty; and in many instances a satisfactory analysis is impossible.

But however difficult the inquiry, it is one of deep interest to pathology, and promises to be not less fruitful in important improvements in obstetric practice.

Our endeavour, in this article, will be to paint the actual state of our knowledge of the pathology of the fœtal investments, and of the developed placenta in particular. The result, if we succeed in this object, will be to indicate the directions in which further advances may be usefully sought.

The anatomy of the fœtal membranes and the placenta has engaged the

attention of many celebrated men. Although it cannot be said that this, the fundamental element of our inquiry, has been finally settled, or that on several points there is not still verge for controversy, it may nevertheless be asserted, that the cardinal points have been demonstrated with sufficient certainty to admit of a successful investigation into the pathology of these structures. For example, the weight of authority in support of the doctrine of the Hunters, that there is no direct passage of the maternal blood into the vessels of the fœtus, and that the placenta is essentially a double organ, partly an offset from the mother's system, and partly an offset from that of the fœtus, is now so preponderating, that we may safely discard the theory of Galen and Fabricius as inconsistent with well-determined anatomical facts, and as obsolete, notwithstanding the occasional attempts which have been made, down to a very recent date, to revive it.

The admission of this doctrine of the Hunters as a recognised truth, will instantly place the functional importance of the placenta in a strong light: for whilst it was believed that the regenerated blood of the mother ran in uninterrupted channels into the fœtus, and that the deteriorated blood of the fœtus found an equally direct escape into the circulating apparatus of the mother, the office of the placenta could only be regarded as of very subsidiary importance. On the other hand, if it be established that the vascular systems of the mother and the fœtus are individually perfect and distinct from each other, then it follows that all those vital changes which have to be effected in the fœtal blood to fit it to support the life, and provide for the development, of the embryo, must take place at the point of contact of the maternal and fœtal vascular systems, that is, in the placenta. Disease is perverted function. The seat of function must also be the seat of disease. If the placenta is of so great physiological importance, it cannot be of less importance in pathology. Again, if the structure of the placenta, the physical medium of its functions, be diseased or degenerated, then we must necessarily expect to find those functions impaired or destroyed.

The observations of Weber, Sharpey, Reid, Dalrymple, Goodsir, concur in establishing the leading fact of the Hunterian doctrine. Their points of difference amongst themselves in no way invalidate the main conclusion. Indeed, however obscure may be the anatomy of the structure connecting the utero-placental arteries, and the utero-placental veins or sinuses, the perfect individuality of the fœtal circulatory apparatus is a matter of easy demonstration. The umbilical arteries may be traced down to the *terminations* of the placental tufts; and the umbilical veins may be seen arising from these tufts as their source. There is perfect continuity. Now, if it be proved that *one* circulatory apparatus is complete in itself, and has no direct open communication with the other, then it follows, as a necessary consequence, that that other must also be complete.

The now-received doctrine of the anatomy of the placenta may be thus briefly stated:—The organ is made up of a maternal and a fœtal portion. The maternal portion consists of the utero-placental vessels, arteries, and veins, preserving an unbroken continuity: these vessels, on entering the placenta, push before them a thin investment of decidua, a membrane

derived from the inner wall of the uterus. On the other hand, the fœtal portion consists of the fœtal-placental or umbilical vessels, arteries, and veins, also perfectly continuous: these, which branch out from the umbilical cord on entering the placenta, carry before them an investment of chorion, the external membrane of the ovum. In the substance of the placenta these two portions, the maternal and the fœtal elements, meet in apposition. The changes effected between the blood circulating in these separate systems, are wrought through the walls of the containing vessels.

So much is generally admitted; and we conceive that we may usefully abridge this article by omitting any detailed analysis of the labours of the various anatomists which illustrate the minute structure of the placenta.

But there is one anatomical question which is still keenly contested, and which, as it has a direct bearing upon the object of our present inquiry, the pathology of the placenta, it is of importance to settle. What is the real nature of the *decidua?* If it be a maternal structure, as the weight of evidence proves, it is still an important question to determine the exact nature of its relation to the uterus. About this latter point there has been greater diversity of opinion. Out of this diversity it will be our endeavour to draw a consistent and definite solution. The necessity of this task will be seen, when it is remembered that altered conditions of the decidua, in connexion with abortion in the early months, are frequently observed; and that, therefore, upon the right understanding of the maternal source and exact nature of the decidua must depend the accuracy of our conclusions as to the origin of those changes or conditions which led to the abortion.

Until, then, we have determined whether the decidua is maternal or fœtal, we cannot settle the important preliminary question, whether abortion from disease of the decidua must be assigned to a maternal or to a fœtal cause. Again, unless we can arrive at some distinct notion of the true nature of the decidua, we are precluded from reasoning upon many of the relations of diseases of the decidua to the pathological conditions of the mother and the fœtus.

There are three leading opinions concerning the nature of the decidua, which we may usefully discuss:—1st. The opinion of William Hunter, which has still many adherents, that the decidua is a newly-developed membrane, secreted by the mucous coat of the uterus; 2nd. The opinion adopted by Sharpey, Coste, Goodsir, Robin, Bischoff, and others—viz., that the decidua is simply and truly the mucous membrane of the uterus, in an exalted state of development; 3rd. There is the opinion advocated by Dr. Lee, that the decidua is not of uterine origin at all, but properly a membrane of the ovum, acquired in its descent from the ovary. The most convenient course will be to simplify the case at the outset by eliminating, as may be readily done, the opinion of Dr. Lee. Dr. Lee advances the following arguments against the Hunterian view, and in favour of the embryonic character of the decidua:—1st. He adduces observations of tubal gestation in which no decidua was found lining the uterus; 2nd. He describes a case, and refers to other cases, of tubal gestation, in which the decidua was found covering the ovum in the Fallopian tube; 3rd. He describes and figures a gravid uterus at the second month, in which the ovum was attached to the inferior segment, and not to the fundus, of the uterus.

"The decidual membrane," says Dr. Lee, "closely adhered to the inner surface of the uterus. It was divided by two incisions parallel with the long and transverse incisions previously made in the walls of the uterus. The ovum, about the size of a pullet's egg, was situated at the *lower part* of the uterus, altogether *below the orifice of the Fallopian tube*, leaving a large cavity—the decidual, between the upper part of the fundus and the ovum. Into this cavity the *Fallopian tubes opened by palpable orifices*. . . . . . In all diagrams from Hunter, Wagner, &c., the placenta has invariably been represented as adhering to the fundus, and the decidua reflexa has been situated near the cervix, and appearing as if pushed down before the chorion or ovum. But in this preparation, it is obvious that the decidua reflexa could not have been pushed down by, because it lies above or covers, the ovum; and as the ovum enlarged, the *decidua reflexa* must have been forced upward to the fundus, which was lined with *decidua vera*. . . . . If the statements of the above authors were well founded, it would follow that in all cases the ovum would attach itself by the placenta to the uterus, either directly over the edges of the Fallopian tube, or to its immediate vicinity, and *that the decidual membrane would never be found interposed between the placenta and the uterus as it always is*."

Let us endeavour to estimate the value of these objections. The following facts and considerations dispose of the conclusion that the decidua is of embryonic origin, as Dr. Lee supposes. A structure which all observers concur in describing as decidua has been repeatedly seen lining the uterus in cases of tubal gestation, and which, consequently, could not be regarded as an integral part of the ovum. Dr. Lee's supposed negative cases cannot be held to outweigh the positive observations of others; and when he describes a case of tubal gestation in which he found "a soft thick substance lining the uterus," but which, he says, "was entirely different from the decidua of an ovum aborted at two months," we do not think he adduces a very fortunate argument. It could not be expected that the decidua, under the two opposite circumstances described, should be identical in character: indeed, we are disposed to think that the true explanation of Dr. Lee's dissent lies in this circumstance—viz., that his idea of what constitutes a decidua differs from that now generally entertained: he looks for a distinct, peculiar membrane, readily separable from the uterus, such as Hunter described; whilst others recognise in the decidua merely a development of the mucous membrane. If Dr. Lee, therefore, could not determine that this "soft thick substance lining the uterus" was a distinct membrane, however strongly this might militate against the view which regards the decidua as a simple exudation from the mucous membrane of the uterus, it rather lends weight to the view which regards the decidua as the mucous membrane itself in an exalted state of development. In this "soft thick substance" may not Dr. Lee have seen the mucous membrane of the uterus altered to a certain extent under the influence of conception, and which would have been still further altered had the ovum descended into the uterus? But Dr. Lee has observed that, in cases of tubal gestation, the decidua surrounded the ovum lying in the Fallopian tube. What does this fact, undoubtedly true, prove? The Fallopian tube is fulfilling the function of the uterus. It is a structure of exactly analogous constitution. It forms a muscular cavity, and is lined, like the uterus, with mucous membrane. What wonder if, when the ovum takes up its abode in the Fallopian tube, this mucous membrane undergo the same changes as the mucous membrane of the uterus?

With regard to the specimen, the description of which we have quoted at length, and which Dr. Lee appears to consider conclusive against the uterine origin of the decidua, we are at a loss to perceive that it supplies the smallest evidence in favour of his own view, that it is of embryonic origin. How could a purely embryonic membrane be, at this early period, "closely adherent to the inner surface of the uterus," investing its entire cavity, whilst the ovum itself, figured as occupying but a very small portion of the uterine cavity, was situated at the lower part of the uterus? If this specimen proves, as it may be admitted it does, that the ovum does not always, in its passage from the Fallopian tube into the uterus, push down the decidua reflexa before it, it is by no means inconsistent with the views of those anatomists who regard the decidua as the altered mucous membrane of the uterus, and who universally describe the Fallopian tubes as remaining open even after the entrance of the ovum into the uterine cavity. We cannot but coincide in the opinion of Velpeau after examining this very specimen, that the membrane which lined the fundus uteri was really the mucous membrane. Nor are we satisfied that the supposed demonstration of Dr. Lee, of the mucous membrane of the uterus remaining entire after detaching the membrane he describes as the decidua vera, is conclusive. He may have split the membrane. It is almost impossible, by any ordinary dissection, to separate the mucous membrane completely from the uterus.

We may now usefully discuss together the opinion of William Hunter, and that adopted by more recent anatomists. It will be seen that these opinions, whilst equally excluding the view we have just commented upon, are perfectly reconcileable with each other. It is true that Dr. Hunter* described the decidua as appearing, in the earliest state, to be nothing else than an effusion of coagulable lymph; but he, in numerous passages, clearly shows that he was aware of the intimate relationship between the decidua and the uterus. He says in one place, "though the decidua be allowed to be the outer membrane of the secundines, yet as *it is really the internal lamella of the uterus,* we may retain the old language, and say that the outer membrane of the ovum is chorion, and *that the chorion is in contact with, and adheres to, the uterus.*" Von Baer also, whilst concurring with William Hunter in describing the decidua as an exudation, so describes its structure and formation as to leave no doubt of its maternal origin. "Upon examining," he says, "the internal surface of the uterus, I could see the villi of its lining membrane, which in the unimpregnated uterus are very short, remarkably elongated: between these villi, and passing over them, was a substance not organised, but merely effused, evidently the membrana decidua of Hunter. The uterine vessels were continued into this substance, and formed a number of little loops round the villi; thus anastomosing with each other." (The drawing of Baer, representing this arrangement, is a familiar illustration in our text-books.) This, according to Baer, is the early state of the mucous membrane, and of that secondary development of its structure to which the specific name of decidua has been attached. In the later periods, he agrees with Seiler in affirming that what is called decidua has the mucous membrane also attached to it, and that in labour, as also in the abortions

* 1774, Plates; 1794, Anat. Descr.

of advanced pregnancy, the mucous membrane comes away with the decidua. The condition of the uterus after delivery has an important bearing upon this subject. The following is Mr. Hunter's account:—

"I stretched the uterus carefully in warm water, then inverted it. *The remains of the decidua had been melted down and passed off with the lochia, so that the fasciculated stratum of muscular fibres appeared to be bare,* and to make the internal surface of the uterus."

In like manner Sir Charles Bell says: "Upon inverting the uterus, and brushing off *the decidua*, the muscular structure is very distinctly seen."* If the mucous membrane were not identical with the decidua, what had become of it in these cases? This Journal of September, 1853, contains a very valuable paper on this subject by Dr. Duncan. This gentleman indeed questions the fact that the muscular structure of the uterus is laid bare after delivery, a fact strongly insisted upon by Cruveilhier and others; but he clearly states that the mucous membrane which is left is undoubtedly "the remains of the uterine decidua." Weber and Sharpey distinctly ascertained that when impregnation has taken place, the mucous membrane swells and becomes lax, the follicles increase in size and secrete a granular matter, and the capillaries increase in a proportional degree. Dr. Sharpey indeed supposes that there is in addition a deposition of lymph, but he does not regard this as constituting the decidua, which he considers to be the altered mucous membrane. Goodsir describes the decidua as consisting of two distinct elements: 1st. the mucous membrane of the uterus thickened by a peculiar development; 2nd. of a non-vascular cellular substance, the product of the uterine follicles. The former, he says, constitutes at a later period the greater part of the *decidua vera*, the latter, the *decidua reflexa*. The descriptions of these eminent observers have received a striking confirmation from a memoir by M. Coste.† The facts adduced in this paper are so important as to merit quotation *in extenso*. His observations go beyond those of other anatomists in this, that they appear to establish not only the identity of the decidua with the mucous membrane of the uterus, but also that this mucous membrane undergoes at every menstrual period a change perfectly analogous to that which takes place in pregnancy. He says:—

"Among the young women whose bodies I have opened at the Morgue, there were several who had destroyed themselves at the moment when the ovule was in a state of perfect maturation. In all these women, without exception, I found the uterus lined with a mucous membrane so thick, that if the constancy of this phenomenon had not been the guarantee of its normal condition, I should have taken it for a morbid alteration. The membrane was, in great measure, formed of glands opening on its internal surface by orifices visible to the naked eye. Its thickness not only equalled one-fourth or one-third that of the muscular coat, but it formed circumvolutions, or folds, pressed against each other in the uterine cavity. . . . In cases of extra-uterine gestation, this membrane may undergo still greater development; it then forms folds as voluminous as the cerebral circumvolutions. . . . In women who had committed suicide from the twentieth to the thirtieth day after conception, as in those of whom I have just spoken, I have always found the Fallopian tubes communicating freely with the uterine cavity, the mucous membrane thicker, and possessing the same organization, but

* Medico-Chirurgical Transactions, vol. iv. † Comptes Rendus, 21 Oct. 1850.

the ovum, instead of being found in the cavity of the uterus, was *buried in the wall itself of the hypertrophied mucous membrane*. To discover the ovum, it was necessary to incise the membrane, and to seek for it in its thickness."

M. Coste then compares the position of the ovum in the lining membrane of the uterus to that of the eggs of the Pipa in the dorsal skin of the female.

"When the ovum has become buried in the hypertrophied mucous membrane, it gradually increases in size, and distends the cell which incloses it. This cell dilates accordingly, and also grows in proportion, forming on its free side a projection into the cavity of the uterus, and by the opposite side remaining attached to the muscular coat. The projecting portion becomes what anatomists designate as the decidua reflexa; the portion which adheres to the muscular structure constitutes their decidua serotina or placentalis, and the remainder of the mucous membrane is their parietal or true decidua. These three deciduas have, in fact, the same organization as the uterine mucous membrane whence they proceed, and it is only in the progress of development that they lose the characters of this organization. It follows, therefore, that the uterine mucous membrane of which the decidua is formed must exfoliate after delivery. This in reality takes place, and the seventh specimen, taken from a woman who died twenty-four hours after delivery, exhibits the muscular coat almost entirely denuded and stripped of mucous membrane. Lastly, when the lochia have cleared the uterus of all the *débris* of the exfoliated mucous membrane which remained, there is formed on the bared surface a vegetation which regenerates this mucous membrane, and renders the organ fit for a new impregnation."

It is worthy of remark that Dr. Sharpey* had already suggested an explanation similar to that above described by Coste as the result of observation; viz., "that the minute ovum on reaching the uterus becomes *imbedded in the substance of the then soft and pulpy mucous membrane*, and that in its subsequent enlargement it carries along with it a covering of the membrane, which is expanded into the decidua reflexa."

The following case by Dr. Ad. Hannover† presents a confirmation of this view:

"A servant girl died fourteen days after menstruation had commenced. It had entirely ceased. The thickening of the mucous membrane of the uterus (which attends menstruation, and is considered as a sign of the formation of a decidua,) also extended to the membrane of the tube through which the ovum had passed."

Meckel, Ritchie, Paterson, and Warwick also confirm the opinion that the formation of a decidua is quite independent of fecundation, and is coincident with the maturation of a Graafian vesicle. The investigations of Bischoff, of which an abstract is given in the Midwifery Report in the last number of this Journal, supply further demonstration of the truth of this doctrine. But although the identity of the decidua and the uterine mucous membrane be now established, still the name of "*decidua*" is as characteristic and applicable as ever

The uterine origin of the decidua is again enforced by the analogy of the uterine cotyledons in ruminants.

Numerous observations of the reviewer, in many of which he has been aided by Dr. Hassall, entirely confirm this view. In one specimen of a perfectly healthy gravid uterus at the fourth month, taken from a woman who had committed suicide, and which was carefully injected, the

* Müller's Physiology, p. 1580. † Medical Gazette, Oct. 10, 1851.

mucous membrane could be readily separated, leaving the muscular wall bare. The uterine, or outer surface of the membrane, showed everywhere the broken ends of the vessels which had connected it with the uterus. Again, in every example, and these are now numerous, in which we have examined the decidua in aborted ova of the early months, as early even as the sixth week, we have detected on the outer surface nucleated fibres with twisted tails, which could not be distinguished from fibres taken from the inner surface of the muscular coat. The inference to be drawn from this fact has to our mind been obvious, namely, that in the violent disruption of the ovum which takes place in abortion, the hypertrophied mucous membrane, as we must now call the decidua, is torn off from the muscular wall of the uterus. In the early months this process of separation is, as might be expected, owing to the still intimate vascular connexion of the mucous membrane with the uterus, a violent one. In most instances of early abortion, the entire mucous membrane of the proper uterine cavity is torn away. Hence the profuse hæmorrhage which so often attends this condition. In the later months the mucous membrane becomes more and more freed from its intimate adhesion to the uterus; the numerous vessels of large size, which at an earlier period connected the muscular and mucous coats, disappear; the membrane acquires a more and more distinct character, and at the epoch of delivery it is cast off by a much gentler process than that which attends upon abortion. Not that the detachment of the mucous membrane on this event is necessarily completed by the act of delivery and the expulsion of the placenta; portions of it may exfoliate gradually. Our examinations of the mucous membrane, under the form of decidua, and of the muscular structure, have further led us to this conclusion, that there is an easy transition from muscular tissue to mucous membrane. In one specimen, indeed, this transition was remarkably manifest. A woman died on the second day after the expulsion of a hydatiginous placenta. Portions of the uterus, with mucous membrane or decidua attached, were taken for examination. The decidua consisted of loose shreds, exhibiting elongated fibres and nuclei. A piece taken from close to the wall of the uterus showed more of the fibres and less of the nuclei. The wall of the uterus itself consisted chiefly of fibres, with but few nuclei. The characters of the muscular wall and of the decidua bore a close resemblance. There was an obvious transition from one to the other. It would thus appear that the reproduction of mucous membrane after its exfoliation consists in a modified development of the inner layer of nucleated fibres, or more probably of the cells themselves, from which fibres would otherwise have been formed: the ultimate form assumed by these cells being, in all likelihood, determined by their position in the wall of the uterus, or on its free internal surface.

We have now to examine a part of this question which offers greater complexity, and which is in a corresponding degree more difficult of solution. All anatomists who have examined this subject agree that the umbilical arteries and veins, and their connecting capillaries, are clothed with chorion. But the particular structure which must be held to be chorion is a matter in dispute. The late Mr. Dalrymple described as chorion, "the membrane enclosing the vessels and capillaries, and studded

on the exterior by *nucleated cells*, resembling an irregular epithelium."* This membrane, which is figured by Mr. Dalrymple, is the same structure that Professor Goodsir has more recently described as decidua. It is this latter view which has been the more currently adopted. But whether this more general adoption be the result of subsequent verification of the observations of Professor Goodsir, or of the credit given to that distinguished physiologist, may fairly be questioned. The point is one of great interest and importance to determine. We enter upon the task with some hesitation, yet not without confidence. The strongest argument adduced by Professor Goodsir,† is his observation that the external cells of the villi are continuous with the parietal decidua of the placenta. He thus states his observations and conclusion. He dissected the vessels of an unopened uterus at the full time, by opening one of the large veins over the spot to which the placenta was attached.

"At last, in introducing the probe into the falciform edge of the venous orifices, it was found to have arrived at the placental tufts, which could be seen by raising the edges of the falciform edges. Having passed over the falciform edges, the venous membrane suddenly passed to each side to line the great cavity of the placenta. The flat bands, which I described as the spaces enclosed by anastomosing venous sinuses, became smaller, and, on entering the cavity itself, the bands were seen to have assumed the appearance of threads, which passed in great numbers from the vascular edges of the venous openings, and from the walls of the cavity of the placenta, on to the extremities and sides of the villi and tufts of the placenta. The whole mass of spongy substance—i.e., the whole mass of tufts—were in this manner perceived to be attached by innumerable threads of venous membrane to that section of the parietal decidua of the placenta, which was covered by the venous membrane. On proceeding deeper into the substance of the placenta, I perceived that throughout the whole extent villus was connected to villus, and tuft to tuft, by similar threads of venous membrane. Sometimes the apex of one tuft was connected to the apex of another. On minute examination, these threads were found to be tubular, and the membrane of which they were formed was seen to be continuous in one direction with the lining membrane of the vascular system of the mother, and in the other with the external membrane of the tufts of the placenta, and passing from tuft to tuft, so as to form the central containing membrane of the bag of the placenta. These threads, as well as their cavities, are somewhat funnel-shaped at each end. The funnel-shaped portions of the cavities of threads, and in some instances the whole length of the tubes, were found full of cells continuous in one direction with the parietal decidua of the placenta, and in the other with the external cells of the placental villi.

"This led me at once to perceive the real signification of the external cells of the placental tufts. I saw that this great system of cells was a portion of the decidua *all but cut off* from the principal mass by the enormous development of the decidual vascular net-work, but *still connected with it by minute files of cells, which fill the cavities of the placental threads.*"

This is the basis upon which the conclusion that the cellular investing membrane of the umbilical vessels is decidua mainly rests. If the continuity which Professor Goodsir thinks he observed were clearly demonstrated, both by the uninterrupted extension of the membrane from the undoubted decidua covering the maternal surface of the placenta to the umbilical capillaries, and also by a sufficient identity of structure of the membrane as observed in these two different situations, then would

* Medico-Chirurgical Transactions, vol. xxv. 1841-2.
† Anatomical and Pathological Observations. Edinburgh, 1845.

this conclusion rest upon a more satisfactory foundation. But it is certainly deserving of remark, that structures "*almost cut off*" from each other, and apparently only connected by "*cells filling the cavities of minute threads,*" may be of a different nature. It must also be borne in mind that this questionable continuity has been observed in *one* instance only. On the other hand, Mr. Dalrymple could not trace the decidua from the uterine surface of the placenta "further than between the lobules, the extent to which it thus penetrates varying with the extent of the fissure." Nor have we ourselves yet been more successful.

Again, what is the fact as to identity or difference of structure of the membrane investing the villi and the decidua? According to our own observations, made in conjunction with Dr. Hassall, there is an essential difference. The real cellular decidua consists of a fibrous membrane, in which are embedded cells, for the most part having a caudate elongation at either end, and of *large size.* The membrane investing the umbilical capillaries contains cells of a slightly oval form, much more thickly studded together than is the case in the decidua, and much smaller. It can scarcely be admitted that differences so marked can be wholly explained by the modifications impressed upon the development of the membrane covering the umbilical vessels, by its altered situation. Again, this membrane, when in a healthy state, cannot be separated from the underlying vessels, unless the placenta have been kept long enough to admit of an incipient putrefactive change. When it has been removed, either by disease, as it commonly is to a partial extent when affected with fatty degeneration, or by tearing after maceration, no second membrane can be demonstrated between it and the proper walls of the vessel. The vessel itself is immediately observed; or, at any rate, a structure presenting all the characters by which capillaries in other parts are usually recognised. "The walls of the placental blood-vessels, like those of the same diameter occurring elsewhere, are thickly studded with elongated nuclei."* It must be observed further, that the membrane which we have described as chorion, uniformly invests the umbilical blood-vessels, from their capillary extremities along their walls, until they have attained a large size. As it was impossible to trace the decidua *onward* from the uterine surface of the placenta, so is it equally impossible to trace it *backwards* from the umbilical vessels to the decidua. Lastly, the membrane which we have called chorion, may be observed possessing the cellular and other characters described, in ova of as early as six weeks' development, and even earlier; that is, at a period when the chorion, covered with shaggy villi, and the decidua are perfectly distinct from each other, when not uncommonly in the case of abortion it happens that the fœtal portion of the ovum, with its chorion, comes away one day, and the maternal portion, the decidua, comes away on the next. With a view to the determination of this question, we have submitted several specimens of very early ova, kindly supplied to us by Professor Sharpey, to examination. In these specimens, the structure of the outer membrane of the villi of the chorion was plainly identical with that of the outer layer of the chorion itself. In short, this investing membrane of the villi was traced back to the chorion.

* Dr. Barnes' first paper on Fatty Degeneration.

The objections to Professor Goodsir's view may be summed up as follows:

1. The structure of the membrane investing the umbilical capillaries, which he describes as decidua, is essentially different from that of decidua observed elsewhere.

2. The continuity of this membrane with the decidua is not clearly demonstrated.

3. This membrane adheres in a very intimate manner to the underlying tissues of the vessels

4. When it is stripped off, no intermediate membrane between it and the walls of the vessels is observed.

5. The structure which lies immediately beneath it bears all the characters of vessels of a similar size occurring elsewhere, and must therefore, for this reason, and (4), be admitted to be the proper wall of the umbilical capillaries, and not the chorion, which, according to Professor Goodsir's view, it must necessarily be.

6. This membrane may be observed in very early ova *to be continuous with the outer layer of the chorion.*

We conceive that we are now fairly justified in drawing the following conclusions: 1st. That the decidua is a membrane of maternal origin; 2nd. That it is nothing more nor less than the mucous membrane of the uterus itself in an altered state of development. We may also observe that there is nothing in the descriptions of the decidua of Hunter and Baer which is inconsistent with this latter conclusion. Surely we may practically regard as one membrane, and that the mucous membrane, the thickened membrane, consisting of elongated villi, between and over which was spread an effused substance, into which the uterine vessels were continued; and which they both agree in stating comes away altogether, leaving bare the muscular coat after delivery. 3rd. That the cellular membrane investing the umbilical vessels belongs to the proper fœtal system, that it is truly chorion.

The recognition of these conclusions will, as we have already stated, throw a new light upon the pathology of abortion, enabling us to assign to a maternal cause those cases in which the decidua is primarily diseased, and limiting the range of fœtal causation to those cases in which the true fœtal structures of the placenta, those pertaining to the chorion and umbilical vessels, are found affected. No attempt at all systematic or successful has hitherto been made to establish this first and essential division.

As it is one of the principal objects of this article to bring together such a summary or digest of what is known upon this subject, as will serve to expose the present imperfect state of our information, and lead the way to future advances, it will be useful to pass in rapid review the chief contributions that have been made to the pathology of the placenta. The general character of these contributions is the want of precision, of the requisite minuteness of dissection and examination, of any reference to systematic arrangement. There is a mass of isolated facts, many of them inaccurately observed and related. Indeed, the most efficient instrument of analysis, the microscope, had seldom or never been turned to account in unravelling the various and complicated morbid conditions

of the placenta, until the investigations of the reviewer were commenced. Hence very many of the reports describing particular lesions of the placenta, and many of the general deductions that have been made regarding the pathology of this organ, can have but little value. The naked eye can only take cognizance of the gross appearances, and can only lead to rough and uncertain conclusions, until it has been trained to the more critical and finer distinctions, by that constant education and correction of our unaided sensual impressions, which the microscope supplies. What reliance can be placed upon the numerous descriptions recorded of scirrhus, steatomatous tumours, tubercles, or fibrinous deposits in the placenta, when the characters of the several lesions thus denominated have been only subjected to the naked eye and the scalpel? To attempt to frame any general expressions out of such doubtful particulars, or to raise any systematic structure upon so insecure a foundation, would be a profitless task. It were far better at the very outset of our inquiry to disencumber ourselves of such untrustworthy materials.

We would not, however, be understood to include in this condemnation many valuable monographs, which are evidently the result of careful observation and of legitimate induction. The writings of Murat, Brachet, Dance, Cruveilhier, and others, are justly esteemed; and the elaborate article of Professor Simpson must undoubtedly be regarded as the most important contribution to the subject which had, up to the period of its publication, been made. From the time of Dr. Simpson's paper down to the announcement of the discovery of fatty degeneration of the placenta, no further step had been made calculated to advance in any material degree our knowledge of the pathology of the organ. Not only did this discovery assimilate the pathological history of the placenta to the better cultivated and more scientific pathology of the permanent organs, but it furnished that touchstone which was so much needed, in order to give to the numerous and hitherto often misinterpreted abnormal appearances of the placenta their true solution, to unfold their real pathological meaning. Its importance is not to be measured by this fact, that a new lesion of the placenta, before unsuspected, was added to the admitted list of morbid changes, not alone because it was thus established that the placenta was obnoxious to all those changes which may affect the internal organs of the economy, but chiefly because the recognition of fatty degeneration in the anatomical elements of the placenta supplied the pathologist with the certain means of analysing, distinguishing, and rightly estimating the various other alterations to which the organ is liable. The grosser physical signs of the placentas described by the reviewer correspond closely with those described by numerous authors as examples of scirrhus, fibrinous deposits, induration, atrophy, tubercles, steatomatous tumours. Yet a rigid microscopical investigation has resolved appearances interpreted into all these varied conditions, into simple degeneration of the component tissues of the placenta. What can be the pathological value of conclusions based upon descriptions in which errors so gross may be presumed? Analogy may indeed point strongly to the conclusion that true scirrhus or true tubercle may be found in the placenta; but it is not too much to deny that either of these heterologous formations has ever yet been rigorously demonstrated.

It is manifest, then, that the work of demolition must precede that of construction. Much unsound material, which, if admitted into the new edifice, would impair its solidity, must be thrown aside altogether; and much more must be rigorously tested before it can be safely employed. We think, therefore, that it would serve no useful purpose, but rather tend to the complication and confusion of a subject already sufficiently obscure, to devote time and space to the consideration of the multitudinous reports of cases of abnormal placentas, with which medical literature abounds.

Appearances that have been interpreted under the double disadvantage of imperfect observation of the cardinal facts, and of erroneous physiological and pathological doctrines, cannot be trusted; and if, by the clearer light of recent physiology and pathology, we might hope to see our way through the mist of theories which encumber these descriptions, we cannot hope, by any process of reasoning or criticism, to arrive at the true physical characters of morbid alterations which were originally imperfectly observed.

(*To be continued.*) *Robert Barnes.*

Review III.

1. *A Manual of Pathological Anatomy.* By Carl Rokitansky, M.D. ('Sydenham Society's Translation.')
2. *An Anatomical Description of the Diseases of the Organs of Circulation and Respiration.* By C. E. Hasse, M.D. ('Sydenham Society's Translation.')
3. *Précis d'Anatomie Pathologique.* Par G. Andral, M.D.
4. *Pathological Anatomy.* By Robert Carswell, M.D.
5. *On Cancerous and Cancroid Growths.* By J. H. Bennett, M.D.
6. *On White Patches on the Pericardium.* By James Paget, F.R.S. ('Med. Chir. Trans.' vol. xxiii.)
7. *On the Nervous Centres, &c.* By Robert B. Todd, M.D., F.R.S. ('Cyclopædia of Anatomy and Physiology.')
8. *On Cirrhosis of the Lungs.* By D. J. Corrigan, M.D. ('Dublin Journal," vol. xiii.)

(*Continued from No. 26, p. 384.*)

The morbid change which constitutes the essence of cirrhosis of the liver is clearly of the same kind as those which we have been considering. We shall first quote Dr. Carswell's account of the production of the granular liver, and afterwards proceed to detail some of our own observations respecting certain analogous conditions which are not all attended with the same alteration of shape.

"The liver, when affected with atrophy from this cause, is sometimes reduced to a fourth of its normal dimensions; its consistence generally increases with the diminution of its bulk; it appears shrunk, and has an irregularly rounded form, particularly at its edges, and the whole of its external surface is raised into round flat projections, varying from the size of hemp-seed to that of a pea, or even a small cherry. Examined more narrowly, the round flat projections are found to be

composed of several smaller ones, and these, again, of the individual lobules of the liver; so that the larger projections are formed of aggregated groups of lobules, each separated the one from the other by cellulo-fibrous or fibrous tissue, the quantity of which varies considerably, and is always greatest between the largest groups of lobules. The situation of this tissue, its distribution, the manner in which it gives rise to the tuberiform arrangement of the lobules, and the diminution observed in the bulk of the liver, are important circumstances in the pathology of this affection, and which are most satisfactorily illustrated by a careful examination of the changes which have taken place in the structure of the organ. When that has been exposed by incision, the cut surface presents the same tuberiform arrangement seen on the external surface beneath the peritoneal covering, the lobules being grouped into smaller or larger masses, mostly of a round, ovoid, or pyriform shape. The cellulo-fibrous or fibrous tissue now forms a conspicuous feature in the disease, both on account of its greater quantity, compared with that of the lobular structure of the liver, and the contrast of its white or grey colour with the rusty yellowish, or greenish-brown colour of the lobules. It is seen occupying the sheath of the portal veins, following the whole course of these vessels, both in their passage to, and their distribution between, the lobules in which they terminate. It thus forms around the veins, in the former situation, a firm fibrous sheath; and in the latter, a capsule enclosing a variable number of lobules, in some parts only four or six, in others ten, twenty, or more. Hence the obvious reason why the lobules are grouped together in the form of tumours of different sizes, containing, or subdivided into, smaller ones. In separating one of these groups of lobules or tumours from the surrounding ones, which can often be done with great facility, especially at the commencement of the disease, we find that it is held, at a certain point of its circumference, by the bloodvessels which pass into the lobules contained within it. At this point the vessels are obviously constricted by the fibrous sheath which surrounds them, and the lobules themselves by the same tissue which forms their common capsular covering. The interior of each group of lobules, when exposed by section, presents a number of fibrous intersections, continuous with the common capsule, and obviously formed by this tissue where it surrounds the terminal divisions of the portal veins. The quantity of the fibrous tissue, compared with that of the lobular structure of the liver in this disease, varies greatly. At the commencement, it is small in quantity, and is best seen where it surrounds the veins before they give off their terminal branches, and consequently, where it forms the capsular covering described. In the progress of the disease it becomes more and more abundant, and, at the termination of some cases, forms the greater part of the bulk of the liver. In the same proportion, also, as it increases in quantity, does the lobular structure of the liver disappear, and its bulk diminish; and so much is this sometimes the case, that almost no trace of the natural structure of the organ is observable. The mechanical obstruction to which it gives rise (i.e., the fibrous tissue) is at first confined to the capillary circulations; but when the lobules, in the progress of the disease, are grouped together in the form of tumours, a new obstacle is created which acts on the venous circulation of the liver in general, but more especially on that of the portal veins. For these tumours either compress the portal veins, or, projecting in the direction of their interior, render them so unequal, and, at the same time, so narrow, that the circulation of the blood through them is always more or less impeded, and sometimes almost entirely interrupted. These effects of the tuberiform condition of the lobules are always much greater in the portal than in the hepatic veins, for this reason—that the attachment of the former to the lobular structure of the liver is very loose, on account of their being provided with a cellular sheath, which the latter have not. They also, however, undergo the same changes in a less degree, particularly the inequality of their internal surfaces from protrusion of the tumours. . . . . The reason why the surface of the liver presents a tuberiform appearance is, that the fibrous tissue being attached to the peritoneal covering, pulls the membrane inwards all round the groups of lobules, where it is most

abundant. The central portion of the groups of lobules must, therefore, from this cause alone, become prominent, and must also be subject to further increase, from the accumulation of blood and bile in their vascular structure. With regard to the state of the gall-ducts and hepatic artery in this disease, it is certain that, as they pass to and from the lobules respectively, enclosed in the capsule of Glisson, they must also, particularly the former, like their accompanying veins, undergo compression. The secretion of the bile, however, is effected, although apparently reduced in quantity, without presenting any remarkable alteration of its physical properties."

Rokitansky remarks, respecting the alteration of form in cirrhosis, that—

"It is always accompanied by a considerable diminution of size; the granulations and the atrophy generally commence at the edges, and the latter attains its extreme development at this point; the edges consequently appear very much thinned, and at last form a mere seam, consisting of cellulo-fibrous tissue, which is contained between two condensed laminæ of peritoneum, and reflected over the convexity, or inverted into the concavity of the liver. The left lobe of the liver is frequently shrunk into a very small flattened cellulo-fibrous appendix, and the thick hemispherical globular mass of the right lobe represents the entire organ. Occasional exceptions arise from the granular disease being developed in a liver that was previously affected by some other disease, as by the fatty degeneration; in this case, the reduction in size only takes place very slowly; and the edges, instead of being thinned down, are often thickened and rounded."

The rest of Rokitansky's account of cirrhosis is too overloaded with detail, and, in part, too incorrect, to make it worth quoting *in extenso*. Some points of it we may allude to further on. Before we proceed to some of our own observations, we shall briefly recall to the reader's mind one or two circumstances relating to the structure of the liver, which it seems worth keeping in view.

The lobules consist of nothing but cells and capillaries in the healthy state; there are no discernible gall-ducts in them, nor any areolar tissue. The lobules are invested, at a certain level, by an almost homogeneous strip of the capsule of Glisson; above and below this, they are continuous with each other. The first discernible gall-ducts are found just outside the lobules in the Glissonian sheaths. The intra-lobular venules, and the surrounding capillaries, are always the chief seat of blood-congestion; and the cells in relation with them are commonly the seat of the deposit of yellow pigment. The marginal cells, on the contrary, are extremely often more or less loaded with oil, or fattily degenerated. Nutmeg liver depends on sanguine congestion of the central capillaries, and impletion of the central cells with yellow pigment, the dark tracts or spots thus formed contrasting with the opaque white of the fattily degenerated margins. If anything were wanting to prove that the old naked-eye guesses at structure were more deserving the name of anatomy of the imagination than microscopic inquiry, on which some sceptics have tried to confer the appellation, it would be the history of the opinions held respecting cirrhosis. The microscope shows in a moment, quite conclusively, that the granulations are merely the remains of the hepatic parenchyma; and it shows this, too, which seems rather surprising, that their elements are often much less altered from their natural condition than one would have supposed could possibly have been the case.

In a liver of markedly granular character, which we recently examined, some of the cells were of quite natural aspect, some laden with oil, some with oil and pigment; none of them had any appearance of being atrophied. Dr. Budd, in a case recorded at length in his work on 'Diseases of the Liver,' gives an exactly similar account. This almost seems to imply that the supply of blood cannot be so completely cut off as it would appear to be; and the fact mentioned by the same high authority, that the capillary vessels in the above case were capable of receiving injection from the portal vein, seems demonstrative of the point that the vascular channels are not completely obstructed. We believe that, in the granular form of cirrhosis, the obstruction tells more upon the minute branches of the portal vein than upon the lobular capillaries. The lobules are, indeed, surrounded and compressed, but it is by a pressure from without; their own structure remains healthy, or nearly so. In other forms of cirrhosis, it is very far otherwise. The portal veins, in a case we recently examined, as far as they could be followed by the eye, appeared pervious; the membrane of the vein was healthy, but perhaps more adherent to the wall of the canal than it should be. The capsule of granular livers is not unfrequently almost or entirely free from morbid change. The grouping together of the lobules into the tumours of various size, which Dr. Carswell speaks of, does not appear to be determined by the direction of ramification of the portal or hepatic veins; it rather seems as if there was something capricious about it, and that it depends only on the formation of a larger quantity of fibroid tissue in one part than in another. A thin section, viewed under a low power of the microscope, is very instructive; it shows islets of parenchyma, separated more or less widely by fibrous tissue. The quantity of the latter may vary very greatly, even in cases which present the granulated character in an equally marked manner. Sometimes it forms tracts, equalling here and there almost the width of the lobules; at others, it seems but little more abundant than natural. In either case, we believe it does not penetrate, in the least degree, the structure of the lobules. It appears to be a corollary from the variability of the quantity of the fibrous tissue in equally granular specimens, that the quality of this new-formed structure must be different —that is, that in one case a small quantity of fibrous tissue will cause as much contraction as a larger quantity will in another. Indeed, we know that, in a liver of very large size, the quantity of new-formed fibrous tissue may be very great, the granular character being, at the same time, moderately though decidedly marked. We have met with one or two instances in which the liver, without being decidedly granular, or even nodulated on its surface, was yet manifestly altered from its natural condition in some similar way. It had an irregular aspect, portions of the surface being raised into prominences of varying size, with intervening furrows. The prominences were sometimes paler than natural. In one case, the cells were, in some degree, stunted and starved-looking, but there was no very apparent increase of fibrous tissue, so that it was doubtful, for some time, on what the contraction depended. On careful examination, it was afterwards found that, in the smaller portal canals, there was a manifest increase and thickening of the fibrous tissue, and that the minute ducts had been much atrophied by its pressure, a circumstance

which often does not take place to any great extent when the quantity of fibroid tissue is abundant; so that it seemed pretty certain that the case was one of incipient cirrhosis, peculiar in this respect—that a decided contracting effect had been produced by a slight alteration in the fibrous tissue of the Glissonian sheaths. The general aspect in these cases is, in some measure, more instructive than the microscopic.

The following cases seem of sufficient interest to quote, as illustrating the latent origin and course of these changes, as well as various points of peculiarity, which we shall notice in each individually.*

H. R., æt. 47, an omnibus-driver, admitted into St. Mary's, April 24th. Stout, plethoric; has been a hard drinker; left work only two weeks ago. General health good; subject during the last six months to attacks of bilious vomiting, which he dates from an injury he received in the right hypochondriac region. Liver found to descend low; some ascites. Urine in good quantity till last three days; albuminous, and depositing lithates. Loud systolic murmur at apex. The dyspnœa became more urgent, and he sank, with signs of failing circulation, in about a fortnight. At the post-mortem examination there was found serous effusion in both pleuræ, and in the peritoneum to a considerable amount; the lungs were œdematous and emphysematous; lower lobes of both partly solidified, partly compressed. The heart very large; the aortic valves hardened by calcareous deposit; not efficient; an elongated calcareous deposit on aortic flap of mitral. Liver large; very hard from fibrinous deposit; surface somewhat uneven; patches of fibrinous deposit on surface. Kidneys, right rather small; capsule thickened and adherent; surface rather granular, congested; left in same state. Spleen rather large, measuring eight by four and a half inches; a dozen or more white spots the size of a pin's head on the outer surface of capsule. The capsule of the liver was chiefly thickened in the course of some of the superficial portal veins. The organ was injected with a fine fluid from the portal vein; but none could be made to penetrate into the capillaries, nor into the interlobular veins. The microscope showed that the cells in the mid-part of the lobules remained tolerably natural, only exhibiting here and there a huge single oil-drop; those, however, adjoining the margins for some depth were utterly atrophied, reduced to mere traces, small granular globules, and nuclei. The fibrous tissue was developed in a very marked manner in the smallest portal canals and fissures; there was no obstruction in the larger. The tubes of the kidney were very much broken up. One remarkable feature in this case, is the complete obstruction which existed, with so little appearance of contraction, in the liver; and another, is the atrophy which affected so exclusively the marginal cells. The case may be summed up (speaking judicially) as chronic latent change in the cardiac valves, the liver, and kidneys; the cause unknown, unless it be the habit of drinking.

E. G., æt. 52, admitted into St. Mary's, Dec. 5th. Had palpitation for six years, dropsy for five weeks, before admission. The heart's action was very feeble, and maintained the circulation very imperfectly. Died suddenly about twelve days after admission. The heart was found

* We cannot forbear to acknowledge the kindness of our colleagues at St. Mary's, in allowing us to make free use of the records of their cases.

enlarged; left ventricle hypertrophied and dilated; the valves all healthy. The lower lobes of both lungs much condensed by pleural effusion. The kidneys were in an advanced state of granular disease. The liver was rather enlarged; it was firm and dense; its capsule was thickened in several white patches. Thin sections of the liver, under the microscope, showed great increase of the fibrous tissue of the small portal canals, and less certainly of the fissures, with more perfect fibre-formation than natural. The marginal cells for a varying but considerable depth were in a state of complete fatty degeneration; those in the interior of the lobules were tolerably healthy. In this case, the change in both liver and kidneys seems to have been completely latent; the enlargement of the heart was probably secondary to both. The cirrhosis of the liver was combined with marginal fatty degeneration.

C. D., æt. 37, admitted into St. George's, May 17th, having been attacked by profuse hæmatemesis that morning. Was bilious during the preceding week, and vomited frequently; never had pain in stomach, but a sensation in his throat, from whence blood seemed to come. The hæmatemesis continued, and he died the next day. At the post-mortem examination the heart was found healthy, except some slight sanguine extravasation beneath endocardium of right ventricle. Stomach and intestines appeared healthy, and no abrasion could be detected. The kidneys were slightly indented on their surface, and the marks corresponded to cicatrix-like seams in their cortical parts, which in some places were diminished. Capsule of spleen slightly thickened, and slight amounts of fibrine were collected in the divisions and marks upon its surface, in some places becoming changed into firm opaque patches. The liver was very large, with a rough granular surface, and a very firm-knotted sectional surface, of a yellow or brown parti-colour. Gall-bladder contained much tarry bile. On the capsule, or in its thickness, there were a great number of minute sand-like grains, which did not cause any opacity, and were not associated with thickening. The new-formed tissue interposed between the masses of parenchyma was very apparent to the naked eye; it had a translucent reddish aspect. A section under the microscope showed that there was immense thickening of the Glissonian sheaths, the fibroid tissue being more abundant in some fissures than others, determined the grouping of the lobules into masses of various size. The addition of acetic acid showed that the fibroid tissue contained a multitude of nuclei, some elongated, the majority being round. The cells were not at all atrophied; many contained oil, or oil and pigment. Here there was fibroid change locating itself principally in the liver, but also affecting the capsule of the spleen, and probably the kidneys. The little grains on the surface of the liver appeared to us very suggestive as to the nature of the changes taking place. It is impossible to ascribe them to an inflammation of the capsule; they were evidently simple, minute exudations of fibrinous matter; and thus evidence a tendency in the system to the formation of such growths as might be developed from such blastema. We consider the hypertrophic growth of the fibrous tissue in the interior of the liver to have resulted very much from similar exudation taking place in its texture. We may remark that the morbid process in the liver was evidently going on, not in a stage of quiescence or retrogression,

as evidenced by the multitude of round nuclei contained in the fibrous tissue; there was, however, no appearance of anything that could be regarded as inflammatory action. The case is an instance of chronic fibroid change, quite latent until the occurrence of a fatal secondary affection, and limiting itself particularly to one organ.

The following case is one of great interest; it occurred in St. Mary's Hospital, under the care of Dr. Sibson.

J. S., æt. 30, servant, admitted Dec. 17th. A thin, sallow, freckled man, having lived abstemiously, and having had good health previously. Was well until one month ago, when he began to suffer from loss of appetite, and some sickness, with pain in region of heart. Cough for fourteen days. Legs œdematous last five days. Abdomen rather distended; no pain anywhere. Urine free, acid, albuminous; specific gravity, =1016; pulse, 108. An enlarged gland on right side of neck. Considerable dulness over splenic region. Dulness over lower part of right back, with rhonchi; blowing respiration more on right side than on the other. The urine was not constantly albuminous; a loud systolic bruit was heard on Jan. 7th to the left of the sternum; the right brachial artery was observed to beat stronger than the left; and dulness was noticed over the upper part of the chest to the left of sternum. After this he got weaker; his legs became more swollen. On the 20th he was jaundiced; and died on 25th. The body was emaciated; the surface was everywhere yellow. The pericardium contained eight ounces of serum; the heart was healthy. There was slight deposit of recent lymph on the posterior part of left lung. Both lungs inferiorly engorged with bloody serum; their cut surfaces presenting appearances as of blood being effused in isolated spots; upper part of lungs emphysematous. Numerous enlarged glands in thorax, and many also about the vena cava, just before it passes through the diaphragm. A tumour of botryoidal aspect was attached to posterior surface of upper bone of sternum; it was hard and dense, and surrounded by a number of enlarged glands. The spleen weighed one pound ten ounces and a half; it contained numerous yellow fibrinous masses, some formed patches, hard and dense, of the size of half-a-crown, others of smaller size were soft and semi-fluid. The left kidney weighed nine ounces and a half; the right, eight ounces; they were much enlarged and flabby. A section through the mass in the anterior mediastinum showed an uniform, yellow-stained surface, marked by some vertical furrows; it contained no apparent vessels, and had a firm texture. It consists of an homogeneous, semi-translucent basis substance, imbedding fibres and remains of nuclei, cells, and granulous matter, as well as fat vesicles. There is no evidence of active cell-growth in the mass; it rather appears like a slow fibrinous infiltration. One of the enlarged glands, cut across, presents a perfectly homogeneous, smooth, somewhat glossy surface; its structure consists of fibroid and homogeneous solid basis substance, imbedding numerous nuclear and celloid particles, and giving out a vast deal of amorphous granular stuff. In the spleen, there was a large yellow mass, a manifest block of fibrine, which presented, under the microscope, an homogeneous translucent substance, imbedding remains of splenic structure, some little masses of yellow pigment, and some triple phosphate prisms. The rest of the spleen was solid, firm, and heavy, and mottled over with whity-yellow stained patches, which appeared to be similar

fibrinous infiltrations; it contained much yellow matter (altered atine); many of its nuclei were developed into cells, and very many, o, were producing fibres. The liver was markedly jaundiced, as every other part; and it was especially observable how the colour was chiefly located in the central cells of the lobules. The cells were in general rather atrophied; the marginal contained scarce a trace of oil, and none of pigment. The Glissonian sheaths were greatly thickened, in some parts much encroaching on the lobules. The gall-bladder was empty. The kidneys were very large, pale, and of smooth surface; the cortical tubes contained a bulky granular epithelium, and were, at least many of them, much dilated; they often contained either dark-red remains of hæmorrhagic extravasation, or more often homogeneous fibrinous moulds, tinted of a bright yellow. The granular epithelium was not at all coloured. The medullary tubes were obstructed like the cortical. The Malpighian tufts were, some obscured, others filled with homogeneous fibrine. It seems difficult to read this case in any other way than as one of chronic renal disease, with slow, simultaneous cirrhotic change in the liver. At a later period, fibrinous infiltration seems to have taken place more rapidly in the lymphatic glands, and in the spleen, where it formed at one part a separate mass, as if by an acute tumultuous action. The opinion that fibrine is an effete product, not a highly plastic one, receives much support from such a case as this. The rapid occurrence of hypertrophic enlargement of the lymphatic glands concurring with fibrinous deposit in the spleen, suggests the idea that the same exudation of fibrinous matter, which, in the one case, formed a mere deposit, in the other, gave rise, by undergoing a low grade of development, to hypertrophy.

W. B., æt. 32, subject to winter cough, not emaciated, died in St. George's Hospital, with great prostration of strength, and much mental confusion; surface of body was of dusky colour. Pulse quiet. At the post-mortem examination the lungs were found studded throughout with grey granulations, and in the first stage of pneumonia. Heart healthy; testis tubercular; liver soft and flabby, rather pale, with patches of indeterminately localized congestion, weighed four pounds two ounces. Kidneys congested, contained some small scrofulous deposits; together they weighed one pound. There was much subarachnoid fluid, about two drachms in each lateral ventricle. The cells of the liver were everywhere loaded with oil, and with opaque granular contents; the oily accumulation was very great in the marginal cells. There was great increase of the fibrous tissue of the Glissonian sheaths, which had advanced so much, as to encroach considerably upon the parenchyma, enlarging the spaces and fissures. This encroachment, however, did not in all cases occupy the whole length of the fissure, it involved sometimes only a part; so that there were seen here and there circular patches of new fibroid tissue. The ducts were much atrophied by the pressure of the new-formed material. The especial development of the fibroid tissue at different spots is a remarkable and unusual circumstance. The cirrhosis was combined with fatty degeneration, and perhaps one may say, the scrofulous with a low degree of the fibrinous diathesis.*

* We do not think the term is by any means free from objection, but it may be perhaps conveniently employed to designate that state in which there is a tendency to fibroid change throughout the system.

There are other instances of cirrhotic change frequently met with, in which very little of the granular character is apparent, and which are chiefly distinguished by great increase of density and toughness, various alterations of shape, and the wasting of the parenchyma revealed by the microscope. These may be called, generally, the *non-granular* instances of cirrhosis, although, as in some of the cases just related, where the fibroid development was confined to the Glissonian sheaths, the granular character does not constantly present itself when the parenchyma remains nearly or quite unaffected. The surface is often marked by whitish streaks, which belong to portal canals whose sheath of fibrous tissue is abnormally thickened. Often, also, there are white patches of thickening of the investing peritoneum, but these may exist in a very marked manner with or without adhesions; while there is little or no fibroid development in the interior of the organ.

We may refer here to a communication which we made to the Pathological Society, in 1848, and which was republished in the 'Transactions of the Medico-Chirurgical Society' for 1852, as containing a detailed account of the changes observed in the non-granular form of cirrhosis. This we shall not quote again, but mention briefly the chief points, and leave our readers to the perusal of the subjoined cases for further descriptive details.

These livers often present exquisite specimens of the nutmeg appearance; they are never so shrunk as the granular ones, and often exhibit no remarkable alteration of shape. Their density and toughness are greatly increased, and this is almost their best non-microscopic character. The Glissonian sheaths are thickened in a greater or less degree, and, what is most important, the parenchyma of the lobules is very greatly altered. This alteration consists in atrophy and destruction of the cells more or less complete, and their replacement by an amorpho-granular stromal substance, which contains *débris* of the cells and imperfect celloid particles. It is a very remarkable and important circumstance, that so grave an alteration should have taken place in livers that exhibit, to common inspection, no very marked diseased condition. This circumstance, that the lobules are not merely compressed and atrophied, but involved in the morbid process, is the distinctive feature of the variety of cirrhosis we are now considering. The following cases will illustrate, in some measure, the nature and pathological relations of this state.

M. K., æt. 39, admitted Nov. 1 into St. George's Hospital: has ascites, said to have commenced six weeks ago, without œdema of ankles; has had cough and expectoration last three or four months. Many years ago had an attack of jaundice, with severe pain in right hypochondrium, for which he was very actively treated. Has enjoyed good health, with the exception of occasional slight attacks of rheumatism. Abdomen at present much distended, partly with flatus, partly with fluid; tongue furred; bowels confined; pulse 96, rather weak; urine scanty, high coloured, free from albumen. Nov. 9th, he began to complain of pain in the abdomen, with more distension; after some remission it returned, with a good deal of tenderness and parched, red tongue, and rather shrunken features; after blisters, pain became less severe, but countenance was more shrunk and emaciated; appetite now quite failed; dulness and want of breathing at

base of left lung appeared, and he died on Nov. 26th. The body was not much emaciated; there was a small quantity of fluid in the left pleura, and several old adhesions—the right was obliterated; lungs much congested posteriorly; heart healthy; blood rather fluid; a large quantity of yellowish serum in peritoneum, which was roughened and coated with recent lymph in several parts. The liver was covered over with false membranes, both old and recent; its edges were very much rounded, and its cut surface nutmeggy: gall bladder full of healthy looking bile. Kidneys smooth on surface, not dwindled, but remarkably confused in structure. Spleen adherent to diaphragm. The liver had quite lost its natural structure; it was converted into a mass which seemed to consist of an homogeneo-fibrous basis substance, extending throughout the lobules, and imbedding *débris* of hepatic cells and numerous oil drops. The investing membrane of the portal canals seemed to be certainly thickened in many parts, but not generally; the chief alteration was certainly in the lobules. The cortical tubes of the kidney were much infarcted with granular and oily matter, but they were not broken up. The attack of hepatitis many years ago doubtless produced the false membrane on the surface of the liver, and the peritonitis shortly before death the recent effusion of lymph; but there is no evidence to lead one to believe that the extreme structural alteration of the parenchyma was owing to either. It is presumable that the change must have been a very gradual one, otherwise it would surely have produced some severe effects upon the system. The secretion of healthy looking bile, with such great alteration of the hepatic cells, can hardly be accounted for on the common view of the structure of the liver. The atrophy of the hepatic cells we regard as secondary to the deposition of the homogeneo-fibrous substance in the interior of the lobules. Disease of the heart had nothing to do with the change in this case.

A. C., æt. 32, admitted Sept. 20, into St. George's Hospital. Had hæmoptysis first two years ago, when marching up-hill in South Africa. Previous to this, had two attacks of rheumatic fever, but was not conscious of shortness of breath or palpitation before this seizure, and has constantly suffered from them more or less ever since. Has anasarca of legs, ascites, and flatulent distension of abdomen. Face dusky, eyes heavy, conjunctivæ yellowish; pulse feeble, frequent, regular; breathing oppressed; heart's action increased; præcordial dulness extended; loud systolic murmur, of equal intensity at base and apex; loud râles in chest; urine loaded, and at first slightly albuminous. Oct. 10th, passed much blood by stools; 21st, cough harassing, dyspnœa urgent, occasional severe pain in chest. The symptoms became worse: blood appeared in the sputa, and he died Nov. 26th. The post-mortem examination showed great anasarca of lower limbs, and some of upper; an icteric tint of skin; some recent adhesions in right, and some old in left, pleura—serum in both. Both lungs œdematous, but crepitant in the greater part of their extent; both also contained masses of extravasated blood: those in the right were more recent and extensive, those in the left were some of them of older date, as shown by their less density and their decolorization. Much serum in pericardium, and several small patches of recently effused lymph, as well as a rather large white patch. Heart rather enlarged; walls of right ventricle in parts thickened, those of left not. Mitral orifice so contracted as not to

admit the tip of the little finger. Tricuspid and aortic valves slightly thickened, but quite efficient. Aorta healthy. The liver was highly nutmeggy. The kidneys were rather coarse; the capsule unnaturally adherent; surface not quite smooth. Spleen rather small and firm, and capsule somewhat thickened. Pancreas unusually firm. A large quantity of yellow serum in the peritoneum. Thin sections of the liver, under the microscope, showed extreme congestion of the capillary plexus in the central part of the lobules; while the peripheral was in great part converted into fibroid tissue, containing *débris* of cells. The cells in the central half of the lobules, as far as the congestion extended, were gorged with dark yellow pigment and oily matter. The fibroid tissue of the Glissonian sheaths was greatly hypertrophied; and, indeed, it seemed as if there was a solidified substance extending throughout the lobules. The kidney was in an early stage of granular degeneration: the cortical tubes near the surface were very much broken up. The vesicular cavities of the pancreas were densely stuffed with epithelium. It is possible that the valvular disease may have orginated in the acute rheumatic attacks, but it is equally possible that it came on as a slow change, together with those of the liver and of the kidney. Nothing seems to have marked the invasion of the latter changes, nor to have directed attention to them at a later period; they were quite latent, and that of the liver was only made out by microscopic examination. How many livers have been recorded "nutmeg," without any idea of the actual changes which disease had produced! We recommend this fact to the consideration of those who love to talk of the illusions of the microscope. The effect of the central congestion of the lobules, occasioned by the disease of the heart, was seen in the excessive formation of yellow pigment in that situation, and in that alone. The firm condition of the pancreas evidently depended on the stuffing of its cavities with epithelium. This state seems to us analogous to the infarction of the renal tubes which is so common in Bright's disease. We shall remark further upon it when we come to speak of the kidney, and shall only state at present that we regard it as an indication of the occurrence of similar chronic change. This state of the pancreas, the cirrhosis of the liver, the granular degeneration of the kidney, and, possibly, the valvular disease, we are inclined to regard as affections of a like kind, originating in a like cause—viz., a morbid state of the blood, causing unhealthy nutrition.

G. St., æt. 32, attorney's clerk, admitted Sept. 19th, into St. Mary's Hospital. Health good till within the last eight months; a great porter drinker; never had syphilis. Had hæmoptysis three months ago; cough, with expectoration; urine high coloured, not albuminous; bowels relaxed; œdema of legs considerable. Hæmoptysis recurred, and he died Oct. 13th in a state of exhaustion. The post-mortem examination showed œdema of legs and feet, and of scrotum; abdomen distended by a gallon and a half of yellow serum, with floating flakes of lymph; vomicæ and tubercles in lungs; heart small, but healthy; mitral valve thickened, but efficient; the liver was very dense and hard, its substance dark, with several little specks on the surface resembling tubercles; spleen soft, capsule somewhat thickened; kidney rather large, apparently healthy. The microscope showed that the fibroid tissue in the liver was increased and had

encroached more or less on the lobules; it was rather irregularly developed, more in some parts than in others. The minute ducts were very well seen in it by the aid of acetic acid; they appeared somewhat atrophied. There was a great deal of newly-formed fibre throughout the parenchyma; it formed long lines, traversing the lobules from the circumference to the centre: the fibres were developed from nuclei. The central cells were of a bright yellow colour; those on the margins contained either oil or dark pigmentary matter. The little specks on the surface were commencing granulations; they consisted of pale, atrophied hepatic cells. The cortical tubes of the kidney were extremely infarcted; there appeared to be some fibre-formation in the matrix. The development of new fibroid tissue in the lobules was well marked in this case, and contributed no doubt to the density of the liver, and to the obstruction of the circulation through the portal vein. The process by which this new tissue was formed does not appear to have been at all inflammatory, nor can any reason be assigned for the unusual circumstance of fibre being developed within the lobules. One can only speak of it as a peculiar perversion of nutrition. The association of tubercular disease with cirrhosis we have before noticed; it was present also in this case, but is not, we think, a common occurrence.

E. L., æt. 61, laundress, admitted Oct. 31st, into St. Mary's Hospital. Her breath had been short six or seven years; dropsy appeared in the feet three weeks ago; urine albuminous; loud systolic bruit; dulness and muco-crepitant râles in right apex; the circulation failed, and she died Nov. 22nd. At the post-mortem examination the heart was found little if at all enlarged; there was some thickening on one curtain of the mitral valve; there was a large cavity in the right, and a smaller one with tubercle in the left, apex of the lungs. Some serous effusion into the peritoneum. Liver much deformed, the various lobules being connected together only by narrow slips of hepatic tissue; the capsule was much thickened, presenting a whitish appearance over the whole surface. There were ten gall-stones in the bladder: its tissue was indurated. Kidneys of lobulated form; structure confused, cysted; capsules somewhat adherent. Three ulcers in the stomach. The liver was exquisitely nutmeg; the central parts of the lobules quite of a deep red; the marginal third of a whity buff colour, the limit between the two was exceedingly defined. Throughout the congested part there were cells, or the remains of cells, thoroughly filled with pigment granules. These were also scattered through the marginal portion, in which there seemed to be a quantity of oily matter diffused among the cells with simultaneous formation of fibrous tissue; there seemed, indeed, to be a network of the latter in each lobule. The cortical tubes of the kidneys were excessively infarcted, containing, here and there, large oil-laden cells; some of the Malpighian tufts were shrunk and dwindled. The change in both liver and kidneys, in this case, was evidently chronic and latent, and independent of cardiac disease. The remarkable thinning of the liver at the parts where the lobes join each other, is a circumstance which we have observed in other instances of cirrhosis; but we do not know that any satisfactory explanation can be given of it. It probably depends on the greater degree of cirrhotic change at those points than elsewhere.

C. K., æt. 57, admitted April 14th into St. George's Hospital, enjoyed good health till seven months ago, when she had jaundice followed by swelling of abdomen, commencing on left side; last month some anasarca of ankles; tongue coated; bowels relaxed; abdomen enlarged, tense, fluctuating; urine not albuminous, loaded with lithates; tumour felt in region of spleen, and also in that of liver; she gradually sank, and died on the 26th. At the post-mortem examination there was observed great anasarca of the legs; the peritoneum contained a very large quantity of amber serum, and some recent lymph; it was rather vascular generally. The great omentum presented several tuberous or granular patches from deposit of fibrine, and was thickened and indurated, and of a greyish ash colour. Edges of liver somewhat rounded; it was much enlarged, weighed six pounds five ounces, and reached even to the spleen as it lay in the abdomen; it was flatter than usual, and placenta-like and very indurated, slightly granular on the surface, and variegated in colour with occasional thickening of its capsule, and was adherent on the under-surface by old bands to the stomach: it contained but little blood, and was exceedingly firm; bile very dark; spleen was adherent to walls of abdomen by false bands, and was of enormous size and weight, ten and a half inches in height, and fifteen in transverse circumference; its capsule was thickened in places and opaque, and on removing it the vessels and connexions yielded with scarce any resistance: it weighed three pounds six ounces: on section it was found to contain scarce any blood, and to be very firm and solid: at its lower part there was a large mass of fibrine cropping up to the surface. The kidneys weighed together seven ounces, and were small, firm, and solid, their capsules rather adherent, and their cortical parts of a somewhat yellow waxen tint, contrasting markedly with the cones. There were also one or two yellow miliary deposits in the cortical parts, in which the microscope showed only the ordinary fibrinous element. There was extensive atheromic deposit in the descending aorta, here and there ossified. The lungs were congested and œdematous. The heart weighed seven ounces; appeared healthy; there was a little thickening in the margin of the mitral, and some atheroma in one or two parts. The cut surface of the spleen was of a peculiar grey uniform aspect, dotted over with pin-point blood specks, and a few of larger size. It consisted of an homogeneo-granular basis substance, having slight tendency to fibrefy, and with numerous nuclear corpuscles and much oily matter dispersed through it. The nuclear corpuscles resembled exactly the natural ones of the spleen. The change appeared to consist in infiltration of the parenchyma with solidifying fibrine, and the large fibrinous mass to be only a more tumultuous form of the same process. In the liver scarce any of the natural cells were to be seen, a few only here and there: they were replaced by a material consisting of numerous nuclei and imperfect celloid particles, lying in a fibro-homogeneous basis substance; this occupied five-sixths or more of the lobules, and was quite pale and bloodless: only in the centres of the lobules were there streaks of red injection; and here only was there yellow pigment in the form of granules agglomerated into globular masses. The Glissonian sheaths were not notably thickened. The toughness and firmness of both liver and spleen were greatly increased. Our friend, Dr. Ogle, to whom we were indebted for the specimens of the

liver and spleen which we examined, has made a communication respecting this case to the Pathological Society, from whose report for 1851–2, p. 335, we quote the following highly important and truth-like observations. Dr. Ogle maintains the following position:

"That a certain condition may exist, either of the blood itself, or of its distributors, the capillary system generally, in which a tendency to the exudation of fibrine, with or without some of the colouring matter of the blood, specially results; the exudation taking place into the various tissues and organs, and not infrequently into several at the same time: as the lungs, liver, kidneys, spleen, &c. It is probable that this tendency will be found to be the interpretation of many instances of secondary inflammation, so-called, arising in the course of other affections, and of those following injuries and operations, though these are generally attributed to purulent infection: also of many instances of extravasations of blood into the substance of the brain or lungs, which often coincide with, but are not possibly, as supposed, dependent upon certain affections of the valves of the heart, which affections are obviously the result of an alteration in the blood's composition. It is probable also that the various atheromic, steatomatous, and calcareous deposits, producing such dire effects in the walls of arteries, are the results of excessive fibrine in the blood, which has become precipitated; and many diseased conditions of the kidney also appear connected with some such tendency; at any rate the visible fibrinous exudations demonstrated in so many cases, and known as fibrinous casts of the urinary tubes, lend some weight to such a supposition."

A somewhat similar view had been propounded by ourselves in a paper read before the Medico-Chirurgical Society about the same date; to this we shall subsequently refer in our concluding general remarks, and will only observe here that it seems almost certain that a chronic deposition of fibrine must have been going on for a length of time in the liver and spleen, and perhaps also in the omentum; that this coincided with degenerative disease of the kidneys, and consequent dyscrasia of the blood, and cannot be supposed to be connected with the peritonitis, which was probably itself a secondary result of the renal disease. The exudation remained chiefly in the form of a deposit, and did not advance to the development of a growth; it did not cause a true hypertrophy of fibrous tissue near which it was effused.

In a liver which we had an opportunity of examining, through the kindness of our friend, Dr. Sieveking, there existed the following condition. It contained numerous nodules and masses of a hard whitish substance, in some measure resembling the aspect of cancer. These varied in size from the area of a shilling down to much smaller dimensions, and were most definitely limited. In other parts there were irregular diffused patches. The unaffected part of the liver was extremely fatty; the arrangement of the cells exceedingly plexiform; there was a moderate amount of thickening of the Glissonian sheaths. In the defined patches there was no appearance at all of cancer cells, only an abundance of fibre-forming solid blastema, imbedding remains of hepatic cells and masses of orange-coloured pigment. Some portal canals were well nigh blocked up by the solid fibrinous masses. In the diffused patches the condition resembled more that of the above-described form of cirrhosis; the fibroid tissue spread itself out between the lobules, and caused great widening of the interlobular fissures. One of these measured one forty-eighth of an inch in width. A mesenteric gland from the same case was

hypertrophied and indurated; it consisted of a dense fibroid tissue imbedding a tolerable number of nuclear and celloid corpuscles having nothing of a cancerous character, and probably being only the remains of the normal structure.

If the morbid formations in this case should have been at bottom cancerous, which on the whole is, we think, most probable, it affords an interesting instance of an almost transitional stage between actual cancer and innocent fibrinous formations or fibrous growths. The blastema, in consequence of a low degree of vegetative power, did not develop cells or nuclei, but remained in a condition little altered from that of simple solidification. The shape and aspect of the masses betrayed the latent cancerous tendency, which it is quite conceivable might have been roused into malignant activity by some cause affecting the system generally. It is particularly interesting to remark that the morbid change in certain parts (the diffused patches) imitated exactly that occurring in cirrhosis. We shall meet with some instances analogous to this one when we come to speak of the stomach.

It seems to us pretty clearly proved by the above cases, that in certain states of system there exists a strong tendency to the deposition of a fibrinous or fibrino-albuminous material in the tissue of various parts; that this solidifies, and produces a stroma or basis substance, which may be more or less granular, or homogeneo-granular, or fibrillating; containing, besides *débris* of the involved natural structure, a varying proportion of nuclear corpuscles. Evidence to show that this deposition is the result of inflammation, seems almost or altogether wanting; in some instances it seems pretty certainly attributable to a deteriorated state of blood; invariably the process is latent, and only betrayed by its secondary effects. There seems to be an evident relation between the deposition of fibrine in quantity, with but feeble indications of organization, and the hypertrophied growth of normal fibrous tissue. Both are frequently associated together, as in some of the above cases. The large quantity of blood which the liver receives, and the comparative tardiness of its course, account in some measure for the frequency of deposits, and of chronic changes of the kind reviewed in this part.

Rokitansky gives no account that we can find of any change analogous to that we are considering taking place in the lymphatic glands; neither does Hasse: but in Andral's 'Précis d'Anat. Patholog.'* (a book which, although belonging to an earlier period of science, we love to turn to for its fidelity to facts, its ample collection of instructive instances, and its sound judicious principles) we find the following description:

"Instead of owing their increase in size to a simple congestion of blood, the lymphatic glands may hypertrophy, and then they present an increase of size and hardness, which coincides sometimes with a reddish grey or brown colour of their tissue, and sometimes with a complete decoloration of this same tissue. White induration of the lymphatic glands constitutes what is called their scirrhous state; one finds then in them nothing except an homogeneous tissue; hard, *crying* under the scalpel, of a dull white, or glistening like mother of pearl. In the midst of this tissue appear sometimes injected vessels, which are remains of the natural structure, and not of new formation."

* Tom. ii. p. 450.

We subjoin some of our examinations of glands in this state. The trachial, bronchial, and lumbar glands were considerably enlarged in a man who died with *quasi-tuberculous* infiltration of the upper parts of both lungs with small commencing cavities. We say quasi-tuberculous, because the material of the infiltration really seemed to be intermediate between tubercle and fibrinous matter. The bronchi were inflamed, those of the left lung uniformly dilated (a further proof of the contractile fibrinous character of the infiltration), and contained cylinders of muco-puriform matter. There were numerous sub-pleural cartilaginoid nodules in both lungs, and a few similar ones in the liver. The kidneys were tolerably healthy, but their capsules were morbidly adherent. The enlarged glands had very much the aspect of scirrhus, and were very firm. They consisted of multitudes of nuclei set close together in an homogeneo-granular basis-substance, with some celloid particles. Acetic acid rendered the basis-substance translucent, and brought into view many elongated nuclei. There was little or no development of fibre. A tumour of about the size of a walnut, removed from beneath the lower jaw of a young female by our colleague, Mr. James Lane, was of a grey, semi-transparent, glossy aspect; it showed under the microscope multitudes of nuclei, which disappeared on the addition of liquor potassæ, and allowed a fibroid stromal substance, in which they had been imbedded, to come into view. The nuclei in this and in the preceding case were doubtless the natural elements of the gland; the cause of the hypertrophy was the fibrino-albuminous plasma which had solidified among them. We have seen several other instances of a like kind, one of which (that of J. S.) we have already recorded. It does not seem to us difficult to distinguish this condition from that produced by inflammation, tuberculous deposit, or cancer; the firmness, the non-vascularity, the absence of *suc cancereux*, and the microscopic characters, furnish sufficient grounds of distinction, at least in the great majority of cases. The one instance where the diagnosis is doubtful is that of cancer with very little cell formation, such as we have noticed above; here it may be often extremely difficult to pronounce whether the hypertrophic enlargement of an absorbent gland is of a simple kind, or whether it contains a cancerous principle. The question is one, in fact, which cannot be settled by inspection of the dead structures, but only by the behaviour of living ones. The presence of unquestionable cancer in any one part would make it morally certain that the *per se* doubtful formations in other parts were cancerous too.

In the kidney we find certainly no decided tendency to the production of fibroid changes, but rather the reverse. Almost all the best observers have expressed themselves against the view that such constitutes any essential part of morbus Brightii. Henle is the only authority who ascribes much importance to the production of new fibrous tissue, and he is charged by Dr. G. Johnson with having mistaken the normally existing matrix for a morbid product. Even Frerichs, whom we quote, and with whom we completely agree, has given a figure to illustrate the fibroid change, which seems liable to cause misapprehension; it may be that he intends it to represent a thickened matrix, but it is certainly, as Dr. G. Johnson remarks, "very like a correct representation of the

fibrous matrix as it exists in the medullary cones." We would observe (*en passant*) that it is rather the fibrous character of the matrix that Frerichs doubts, than the existence of one. Mr. Simon and Virchow agree in discrediting the essentiality of any fibroid development to the production of granular kidney, and explain the appearances which have been observed as being occasioned by the remains of the basement membrane of the collapsed tubes, and the more manifest appearance of the matrix in consequence of the atrophy of the glandular structure. Frerichs remarks that, with respect to most cases, this view remains correct, but that there are, however, exceptions, in which no doubt can be felt respecting the new formation of connecting (areolar) tissue. In describing the condition of the large mottled kidney (his second stage), he speaks thus: "The exudation in the interstitial tissue is subordinate, and is often completely wanting." In sections of hardened kidney rendered translucent by some reagent, "the interstices between the tubules are seen, for the most part, just as they are in normal organs, but sometimes they are wider in consequence of infiltration of the interstitial tissue with amorphous exudation." We have ourselves observed this intertubular infiltration. With regard to the contracted granular condition of kidney, Frerichs believes that in some cases the formation of fibroid tissue takes place in a more marked manner, and in this also our observation coincides with his. He says, "the results of our examination present themselves in a somewhat different manner when a part of the exudation in the interstitial tissue of the kidneys changes itself into connecting tissue, which, by its shrinking together like a cicatrix, exerts a compressing influence on the neighbouring parts, and thereby accelerates the atrophy."

We find in this case, besides the described alterations of the tubuli and of the Malpighian bodies, which in every instance of morbus Brightii are the essential anatomical substratum, new-formed connecting tissue between the urinary canals, and around the capsules. As a general rule, this consists principally of elongated fibre-cells, among which are found completely developed fibrils, which can be isolated, and distinguished most certainly from the remains of the basement membrane of the urinary canals. The Malpighian capsules are surrounded by the new-formed connecting tissue in concentric layers, which are usually $\frac{1}{100}$ to $\frac{1}{75}$ line thick. Very commonly there is also a portion of the exudation organised into fibre-cells within the capsules; these cells advance up to the vascular loops of the glomeruli, and deposit themselves between these. The urinary canals are firmly enclosed by the new-formed connecting tissue; there are produced between them greater and smaller interstices, which can be followed on longitudinal sections, but best on transverse ones, of the renal substance. The interstices appear especially widened in those parts where the urinary canals have perished in larger numbers, where thus the remains of the basement membrane, the capillaries of the interstitial tissue, and the new-formed fibre-tissue are fused together into one mass. Very often great oil drops lie imbedded in the fibre-mass, arranged separately or in rows.

We have observed distinctly the formation of fibres originating from nuclei as they are figured by Frerichs, as well as numerous elongated

nuclei, and we think we have also seen something like the envelopes surrounding the Malpighian capsules which he speaks of. We feel, however, quite assured that this occurrence is only accidental, and in nowise of the essence of the disease, though we conceive it may afford a hint as to its real nature. Frerichs' remarks on the view that would class morbus Brightii and cirrhosis of the liver together as identical processes are perfectly just; he allows that there exist between them many analogies, that they concur not seldom in the same individual, and appear to have a common exciting cause in diseases of the heart, but shows clearly that they cannot be considered homologous on the preceding grounds. What kind of relation may subsist between them we shall presently inquire. It is certainly a remarkable circumstance that the kidney should be so inapt to the development of fibroid tissue, while the liver is so prone to it. To search for the cause of this difference seems useless; we can but regard it as a fact. As, however, we have seen that a process does occasionally take place in the kidney, apparently as a part, though not at all an essential one, of the degeneration constituting morbus Brightii, which is quite identical with that form of cirrhosis which gives rise to the granular liver, we may find ourselves justified in inquiring whether the essential degenerative process in the kidney has any correspondent in any of the changes allied to cirrhosis which occur in the liver. This inquiry seems the more reasonable, as it is but just to believe that between the two most important glandular organs there can hardly exist any great and absolute differences, such that the pathological history of the one should be altogether unlike that of the other.

In several of the cases of cirrhosis above recorded, we saw that the principal change consisted in the effusion of an unhealthy plasma in the substance of the lobules, which, solidifying there, caused, or coincided with, the atrophy of the normal cells. No change exactly like this occurs in the kidney, but we think the essential degenerative action of morbus Brightii manifests some very real affinity to it. We believe this consists, like the corresponding change in the liver, essentially in the exudation of unhealthy plasma, which, in the large hypertrophied kidney, is organised into the form of epithelium, distending and choking up the tubes with its abundant growth; and in the small early granulated kidney does not thus become organised, but gives rise at an early period to atrophy of the epithelium and general wasting. This is a simple expression of what is actually observed, apart from theories as to the cause of exudation. In Frerichs' view, everything proceeds from a stage of hyperæmia, plugging up the tubes with fibrinous moulds by the resulting exudation.

Dr. G. Johnson, believing with us the blood to be in an abnormal state, lays it down as a central truth, "that all the changes of structure commence in the secreting cells of the gland, and are the result of an effort made by the cells to eliminate from the blood some abnormal products—some materials which do not naturally enter into the composition of the renal secretion." He thinks that the secreting cells, striving to separate certain noxious matters, certain strange materials from the blood, "become modified in their action and nutrition, and being rapidly thrown off into the tubes, are thence removed by the current of liquid, and appear in an entire form in the urine."

Mr. Simon names the degenerative disease a sub-acute nephritis, and believes that inflammatory action, such as affects a mucous surface, causes the shedding of epithelium in quantity, and consequent obstruction of the tubes with enlargement. When they have in this way been incapacitated from fulfilling their function, atrophy subsequently occurs.

With regard to Frerichs' view we have only to say, that it seems to us repeatedly contradicted by clinical experience and post-mortem examination. From Mr. Simon's we do not much differ, except that we think the exudation is not essentially, nor in most cases at all of inflammatory origin. On Dr. G. Johnson's we make the following remarks. It seems to us sound and correct in this respect, that it regards and accounts for the commonly latent character of the disease, and does not rank it as a mere inflammation or hyperæmia. But it assumes that the blood contains noxious matters which are separable, substances dissolved in the blood foreign to its natural composition, and capable, at least theoretically, of being eliminated from it. It also seems to assume, or take for granted, that the renal epithelium secretes, according to the popular idea, by filling its cells with secretion, and when they are mature allowing them to burst and the secretion to escape. The cells, as Dr. Johnson states, in striving to separate material which is unnatural to them, get altered, and are rapidly thrown off. Now, our view of the abnormal state of the blood is not that it contains certain superadded matters, which make it unhealthy by their presence, but that its own material, its albumen and fibrine, the very things which make it a nutrient fluid, are altered from their normal composition, and are diseased. This diseased blood cannot minister to healthy nutrition.

Again, we see no evidence whatever that the renal cells are in the state of health thrown off after they have filled themselves with secretion; they are, at every period of their existence, merely so much albuminous matter, and there is nothing to show that they even contain urea, uric acid, or any of the other constituents of the urine. The way in which the epithelium performs its functions is quite a mystery; but it seems at least certain that it does not do so in the way which Dr. Johnson supposes. Only thus far we think we may see that, as it must be supposed to be in process of change, and as it does not appear to pass off in the form of the secretion, it must liquefy and be resorbed into the circulating blood. If this did not occur, the channel of the tube would be quickly choked up by the accumulating growth. Something of this kind, we conceive, had occurred in the pancreas before-mentioned, whose ultimate vesicles were stuffed with epithelium. On the theory which we have just hinted at, a correspondence would exist between the change and resorption of the epithelium, and the elimination of the secretion; the two processes would take place in direct ratio to each other, and thus the imperfect performance of the secretory act might be attended with hypertrophy of the epithelium. In other cases, the non-formation, the atrophy, and degeneration of the epithelium would also concur with defective secretion; the albuminous growth not taking place, the materials of the secretion would not be eliminated. Now, these are the two instances of renal degeneration ordinarily met with, and we cannot but think they are better explained on the view we have stated above, that

the fibrine and albumen of the blood are themselves in an unhealthy state, and, therefore, cause unhealthy nutrition, than on the eliminative principle maintained by Dr. Johnson. According to his view, the kidneys are striving to do what they ought, and the business of the physician should be to assist and enable them, and it would be reasonable to hope that in due time all the materies morbi might be got rid of. On our view, the liquor sanguinis remaining in itself unhealthy, might all be eliminated without any advantage; the object clearly would be to make it better. Now there does seem to us to be some resemblance between the condition of the enlarged kidney with its infarcted tubes and that of the lobules of the liver, when they are infiltrated by solidifying exudation. The resemblance is more marked in the process itself than in its ultimate result. In both the essential fault is exudation of unhealthy plasma, but the forms into which it passes in the two cases are not the same. This probably depends on the different endowments of the tissues in the two cases.

The points in which there appears to be an affinity between the diseased states of the liver and kidney referred to, may be enumerated as follow: 1. Both changes are of the nature of degenerations, evidently not the results, as Dr. Prout recognised long ago, of any morbid action that can justly be called inflammatory; 2. Both are commonly latent, and do not attract attention until secondary phenomena begin to appear; 3. Both frequently exist together, and so far from being one the cause of the other, they both seem to be the consequence of some general state of system which is often further manifested by changes of similar kind in other parts; 4. The thickening and adhesion of the capsule of the kidney to the surface, which is a good naked-eye test of the existence of degenerative disease in doubtful cases, is very analogous to the diffused white patchy thickening which is often observed on the surface of the liver in cases of cirrhosis. We conclude, on the whole, that cirrhosis, affecting only the Glissonian sheaths, and not the lobular parenchyma, has no exact analogue in any of the forms of morbus Brightii; but that cirrhosis manifesting itself in both the above situations is very comparable to that condition of the kidney in which there is a production of inter-tubular fibrous tissue, and abnormal increase of epithelium obstructing the tubes.

The glandular structure of the stomach, consisting, as is known, of parallel tubes set vertically to the surface, has not been known to undergo any change depending on the formation of fibroid tissue among its elements. We have, however, recently observed numerous instances in which a morbid process of this kind had been going on. On making a vertical section of the mucous membrane, it was found that the tubes were indiscernible, or that only traces of them were to be seen in the form of mere opaque streaks, while all the intervening and surrounding space was loaded with nuclear particles, and granulous or indistinctly fibroid matter. By acting on the specimen with acetic acid, altered tubes may sometimes be brought into view; at others, nothing can be seen but nucleated substance, very much like that which thickens the Glissonian sheaths in cirrhosis. Drinking does not seem to be the most efficient cause of this condition, though it may probably promote it; out of 3 cases in which the patients were reported to have drank, this change had taken

place only in 1. This change appears to us to be of kindred origin and nature to those we are now considering, and to depend, like them, on the exudation of unhealthy plasma, and disturbance of the normal nutritive actions of the part.

The sub-mucous areolar tissue of the stomach seems to be not unfrequently (according to Andral's trustworthy testimony) the seat of chronic thickening; and though we have no experience of this condition ourselves, we think it will be acceptable to our readers if we quote the excellent description given in the 'Précis d'Anat. Path.,' vol. ii. p. 58:

"Under the name of scirrhus of the stomach or of the intestines, a state has long been described in which nothing else is discoverable than an increase of thickness and density of the cellular membrane which, in the normal state, separates the mucous membrane from the fleshy tunic. One may convince oneself of this by following the hypertrophy of this kind of tissue through all its degrees. First, in many cases of chronic diarrhœas, where the mucous membrane of the great intestine has undergone different kinds of alteration, the cellular tissue which lines it is often found much more apparent than usual; it is sometimes in a case of this kind several lines thick; by itself alone it exceeds the thickness of all the other tunics put together; it is hard, of a pearly white, and destitute of bloodvessels; sometimes there are seen in it fibres or laminæ which have a more or less regular arrangement; sometimes one finds in it nothing but an homogeneous texture, very similar to that of an imperfect cartilage."

He then argues against applying the term scirrhus to what he declares is only a greater degree of the same structural change, but confined to a circumscribed part; he says, in the one case, the cellular tunic has thickened itself as 10 in a circumscribed extent; in the other, has thickened itself as 2 more widely. We can see that this reasoning is not conclusive, both because fibrous tumours are not quite identical with chronic thickenings, and because mere inspection might be insufficient to determine the non-cancerousness of a tumour. He proceeds:

"When one examines the parietes of a stomach or intestine, whose sub-mucous cellular tissue is affected with hypertrophy, one often observes that the hypertrophy is not limited to this cellular tissue: in the thickness of the fleshy tissue there appear white lines, true septa of cellulo-fibrous aspect, which, interposed at intervals between the muscular fibres, isolate them from one another, and cause the fleshy membrane to appear as if lobulated. These septa are continuous, on the one hand, with the sub-mucous cellular tissue, and on the other, with the subserous; they are manifestly nothing else than hypertrophied portions of the intermuscular cellular tissue. But this hypertrophy may become more considerable; instead of simple lines or thin plates, it may happen that one finds scattered, in the midst of the fleshy tunic, hard and white masses, of more or less considerable size, which are nothing else than this same cellular tissue in a state of hypertrophy; these masses increase in their turn, and at length occupy more space than the fleshy tunic, which becomes less and less evident: there comes a time when the most that one perceives, in the midst of enormous masses of indurated cellular tissue, is some muscular fibres scattered widely apart, which are, in some measure, surrounded on all sides by this cellular tissue. At last, all appearance of muscle vanishes, and between the peritoneum and the mucous membrane nothing can any longer be found but a collected mass of cellular tissue, either simply hypertrophied and indurated, or having become consecutively the seat of varied alterations. In the greater number of cases, the hypertrophy of the sub-mucous areolar tissue developes itself as the result of a state of chronic irritation of the mucous mem-

brane, though *none of the numerous varieties of this irritation induces necessarily its formation.* It may happen that one finds no appreciable lesion in the mucous membrane, either because in reality this lesion has not existed for a long time, or even, in other cases, *because it has never existed.* At other times, the mucous membrane is found either simply hyperæmic, or indurated, or softened, or even ulcerated. . . . The sub-mucous tissue, in some cases, is thickened throughout the whole extent of the stomach. Then, on touching the walls of the stomach, one is struck by their hardness; they do not sink down, as in the physiological state; they offer so much resistance to an incision, as to 'cry under the scalpel.' Outside the hypertrophied cellular tunic, sometimes one finds the muscular tunic either in its natural state, or divided into lobules by fibro-cellular intersections, or hypertrophied; or, on the contrary, atrophied to such a degree, that no traces of it can be found. In these cases, the cavity of the stomach is usually of but small size. There are, on the contrary, other cases in which the hypertrophy of the sub-mucous areolar tissue occupies only a circumscribed point of the stomach; sometimes it is one or other of its faces, sometimes it is one of its orifices. Of the different points of the stomach, the one in which the cellular tissue hypertrophies most often is, without doubt, its pyloric extremity, as well as the pylorus itself, properly so called. Sometimes the hypertrophy is exactly limited to the pyloric ring, sometimes it gradually diminishes towards the splenic region. The hypertrophy may form a tumour at the pylorus, which can be recognised through the abdominal parietes; in other cases, it is only evident internally, on opening the stomach. Before the age of thirty-five, this change in the stomach is rare; it is most frequent in the ensuing thirty years of life."

Of course, the question arises immediately, with regard to these instances of fibroid development—especially those which are of circumscribed extent—whether they are of a simple non-specific kind, analogous to such as we have before reviewed, or whether they are of cancerous nature. We think no doubt can exist that both kind of changes may take place; that most instances of general hypertrophy and thickening of the sub-mucous tissue are of a simple kind, such as are commonly ascribed to inflammatory action; that, again, some of the local thickenings, forming more or less distinct tumours, would be of the nature of fibrous growths, while others would differ from both in containing elements more or less manifestly cancerous, and in being associated with unquestionable cancerous growths in other parts. In many of these cases, the distinction would be by no means easy, as in one of the instances we have related above of fibroid masses and diffused patches occurring in the liver.

As bearing, in a very interesting manner, on this question, we quote the following cases from Dr. Bennett's work, with the remarks appended to them:

"Obs. XXI.—Mrs. S., æt. 37, dying with vomiting and exhaustion, a tumour in the left hypochondrium. On dissection, the stomach felt hard, and on opening, it was found greatly thickened throughout its whole extent, but more especially towards the pylorus, where it was nearly an inch thick. On section, the muscular coat was seen hypertrophied, two lines thick. Within the muscular coat, there was a fibrous structure of semi-cartilaginous hardness, generally half an inch in thickness, but at the pylorus, an inch thick. The mucous membrane presented a brownish colour, but was otherwise healthy."

The pyloric, dorsal, and lumbar glands were enlarged, and, to the eye and to the microscope, presented unequivocal evidence of cancer.

"A thin section of the indurated and thickened portion of the stomach was found on examination to be composed of bands of fibrous and elastic tissue, run-

ning sometimes in a straight, at others in a varied direction. Here and there among these might be perceived a very few oval and round corpuscles, varying in diameter from the 1-80th to the 1-75th of a millimetre. They contained a single round nucleus, the largest about 1-50th of a millimetre in diameter. On the addition of acetic acid, the fibrous structure generally became more transparent, presenting only faint lines or striæ, whilst the filaments of elastic tissue underwent no change. The nuclei of the fibrous tissue were rendered very apparent, in the form of elongated spindle-shaped bodies. In this case, the alteration which had taken place in the walls of the stomach was wholly of a fibrous character. No cancer-cells could be detected, and such corpuscles as could be observed resembled, in all their characters, those frequently associated with filamentous tissue. At the same time, the physical characters of the thickened walls of the stomach were such as have hitherto been considered to constitute scirrhus or hard cancer. On this account, I have ventured to call the alteration cancroid. It must not be overlooked, however, that whilst no cancer-cells could be found in the stomach, they existed in great numbers in the mesenteric and lumbar glands. Hence a difficult question arises as to whether the lesion in the stomach bears any relation to the cancerous formation in other structures. Are the oval corpuscles described, free nuclei?

"Obs. XXII.—A man, æt. 44, dies with the same symptoms as in the above case. There was great emaciation; the stomach was much contracted; the coats were firm and resisting, and, on section, were found to be thickened universally, with the exception of a small portion towards the fundus. The cut surface of the coats of the stomach was of dirty-white colour, felt tough and elastic, and thickened to the extent of half an inch in the body of the organ; but towards the pylorus, the thickening amounted to fully three-fourths of an inch. The pyloric orifice, however, freely admitted the passage of the forefinger. The thickening involved the muscular and mucous coats [sub-mucous?] in equal proportions. The internal mucous surface of the stomach was smooth and healthy. The peritoneal covering of the diaphragm presented a milk-white appearance, glistening and indented to the feel, and uniformly thickened to the extent of half an inch. The omentum was shrivelled up, felt firm, had a mammillated appearance, and resembled a round cord, about half an inch in diameter, fringing the larger curvature of the stomach. The peritoneal covering of the intestines and of the mesentery appeared to be sprinkled over with a white granular or pulverulent substance, consisting apparently of chronic lymph, and so firmly attached that it could not be separated without removing the peritoneum with it. In some places, the white indurated matter was aggregated together, presenting an uniform white surface, and giving to the serous membrane an unusually firm consistence. It was evidently the same lesion that affected the diaphragm. Many of the mesenteric glands were of white colour, slightly enlarged, and very indurated, presenting on section an uniform white fibrous structure. On making a section of the thickened portion of stomach with Valentin's knife, it was seen to consist of a dense fibrous structure. On the addition of acetic acid, this became more transparent, and exhibited elongated nuclei very distinctly, at different depths. No isolated cells were to be seen. The sub-mucous tissue presented a similar structure, mingled with a few filaments of curled elastic fibres. The white substance on the diaphragm and mesentery was also composed of bundles of fibrous tissue, presenting the same appearance on the addition of acetic acid. These, however, were mixed with numerous fusiform corpuscles, and what appeared to be free nuclei, consisting of delicate oval bodies, varying in size from the 1-100th to the 1-80th of a millimetre in their long diameter, containing one granule. These bodies underwent no change on adding acetic acid. The mesenteric glands were composed of a similar fibrous stroma, containing also free oval bodies. In this observation we observe the same chronic lesion of the stomach as in the last case, associated with induration of the mesenteric glands, and partial thickening of the peritoneal coat. There are no cancer-cells anywhere to be detected; but, in the last-named situations, there are a number of transparent

oval bodies, containing one granule, unaffected on the addition of acetic acid. They are evidently the same as those seen in the fibrous structure of the stomach in the last observation. Are these oval bodies naked cancer nuclei, or are they, as previously stated, bodies connected with the fibrous tissue? This point can only be elucidated by further observations. In the meantime, I may observe, that having seen them in many deposits, undoubtedly not cancerous, as well as associated with perfect cancer-cells, I am inclined to think they cannot be regarded as necessarily connected with either.

"Obs. XXIII. contains an almost exactly similar history, with the exception of a chronic ulcer of the mucous membrane."

In Cruveilhier's great work, livrais. xxvii., pl. 1, there is a drawing of a stomach taken from a case very similar in many respects to the foregoing Obs. XXII. The following is an abstract of the case. M. F. L., æt. 66, ill 14 months. Her illness commenced with violent pain in the stomach, especially after meals, and followed by vomiting; there was excessive emaciation, no defined tumour in the epigastrium, no special tint of the labia; vomiting occurred at intervals. At the post-mortem examination there were found sub-peritoneal miliary tubercle, and slightly prominent opaline patches; in the great omentum, which was shrunken up, there were white tubercle-like granulations, and the same in the lesser omentum; the stomach was considerably shrunken, its walls were thickened, to the greatest degree near the pylorus, the cardiac orifice was contracted; the mucous membrane had almost entirely disappeared; the submucous areolar tissue, and the muscular coats, were much hypertrophied: both of these coats gradually diminished in thickness from the pyloric extremity to the cardiac. Cruveilhier thinks that the muscular and fibrous coats hypertrophied in consequence of areolar gelatiniform cancerous degeneration of the mucous membrane, and finds in the granulations scattered over the peritoneum and in the two omenta, a decisive argument of the cancerous nature of the disease. With this we cannot agree, the characters of the post-mortem phenomena are very unlike those of colloid cancer: and though only microscopic inquiry could decide, we think the probability is very strong that the affection consisted in fibroid deposit in the serous membrane, and fibroid hypertrophy in the walls of the stomach. There is then the further question, how far these changes were induced by a chronic gastritis destroying ultimately the mucous membrane? From the history of the disease, and from a comparison of this case with Dr. Bennett's XXII., we are more inclined to regard the destruction of the mucous membrane as a secondary than as a primary phenomenon.

In Obs. XXIV. there was a tumour in the right hypochondrium, of recent appearance; the symptoms referrible to the stomach came on only about two months before death. There was much emaciation, and some old tuberculous deposits in the lungs.

"The stomach presented a perfectly white lardaceous appearance, but was exceedingly dense and firm to the feel; its form and size resembled the organ when in a state of contraction. On opening the stomach, its walls were seen to be thickened to the extent of an inch, and this throughout its whole extent, with the exception of a space the size of a five-shilling piece surrounding the pylorus, which was quite healthy. The interior surface of the organ was traversed by several grooves or channels, dividing the hypertrophied structure into clefts, at the base of which the walls of the organ were of normal thickness. The hypertrophied structure occupied the space between the peritoneal and mucous coats;

it was of a glistening white colour, of semi-cartilaginous or tendinous consistence and density, grating under the knife. No trace of the muscular coat could be observed. The mucous membrane was of a light-brown colour in some portions, and of an ash-grey, approaching to black, in others; in a few places its surface was raised so as to form papilla or granulations the size of peas, in other places it was ragged, depressed, and irregular. No ulcerations existed anywhere. Other organs healthy. On examining a thin section of the thickened coats of the stomach, it was seen to consist of a dense fibrous structure, which, on being slightly teased out with needles, was found to consist of an immense number of fusiform corpuscles aggregated together." On scraping the tissue with a knife, there was removed a pulpy substance, which in some places was found to contain, in addition to the fusiform corpuscles, a number of round or oval cells, varying in size from the one-hundredth to the one-fiftieth of a millimetre in their longest diameter. Every degree of gradation might be observed between the oval form of these cells and the fusiform corpuscles; they all possessed a small nucleus, generally about the one-three-hundredth of a millimetre in diameter; no nucleolus could be observed. On the addition of acetic acid, the whole corpuscle was rendered somewhat paler, whilst the nucleus was unchanged. The mucous surface of the stomach was covered with irregularly formed and partly broken down epithelial cells. In this observation the thickening of the stomach had proceeded to an extent which I have never seen equalled, and presented characters quite peculiar. The absence of pyloric stricture and ulceration, the obliteration of the muscular coat, and formation of fibrous tissue in the walls of the organ an inch thick, and the want of cancer cells, are sufficient to raise doubts as to this alteration being strictly cancerous. Two views may be taken of the nature of this lesion: first, that it is a chronic cancer; second, that it is a fibrous growth, resembling a simple structure of hollow viscera. (1) It may be argued in favour of the first view, that in Observation XXI. it was evidently associated with undoubted cancerous disease of the mesenteric glands, and that in all cases of scirrhus the growth is essentially fibrous. The round and oval bodies mingled with the fibres, it may be contended, are nuclei, identical in appearance and structure with the nuclei of cancer cells: and that there are many growths generally considered malignant which only possess these bodies. (2) On the other hand, and in favour of the second view, it may be urged that in structure this alteration resembled in every respect a simple fibrous growth; that if in some cases it be associated with undoubted cancer in other organs, cases also occur where it constitutes the only lesion; that the round and oval bodies are found in many structures which cannot be considered cancerous: for instance, certain exudations round corpora lutea in the ovary, and in soft polypi from the nose; that these latter are more characteristic of fibrous than of other forms of growth; and finally, it may fairly be questioned whether those tumours which only contain these bodies are really cancerous. At the same time, it cannot be denied that the lesion of which we are now speaking has always been considered as scirrhus, and as in its nature malignant. But the idea of simple stricture of the stomach, and hypertrophy of its walls, has not been thought of. When, however, we consider the numerous cases on record of so-called cancer, which have been known to arise from swallowing irritating substances, more especially acrid poisons, we can easily imagine that the effect of such may be to produce simple inflammation and thickening; the same as occurs in the intestines, ureters, and other hollow viscera. This is certainly the view I am inclined to take of this disease,—a view which becomes strengthened on inquiry into the appearance and progress of undoubted cancer of the stomach."

While we agree completely with Dr. Bennett, that the fibroid formation in the last three cases certainly was not of a cancerous nature, we think that his comparison of this alteration to that produced by acrid poisons is scarcely just. We do not think, nor find described in Rokitansky, that

there occur any such considerable thickenings of the submucous tissue after the contact of caustic substances as were found in the above instances. Cicatrices and puckerings are often produced, but not thickening to any great extent. We subjoin Rokitansky's description of gastric fibrous cancer, and the diagnosis of it from hypertrophy of the coats, although we cannot say that we think it throws much light upon the matter.

"Fibrous cancer appears as thickening of the sub-mucous cellular stratum, which congeals into a resisting, whitish, fibro-lardaceous mass, and unites intimately with the mucous and the muscular coats. The latter becomes pale, and gradually undergoes a change, which is characteristic of all kinds of cancer. It increases in thickness, and at the same time degenerates into a pale, yellowish-red areolar tissue, the interstices of which are filled up by a slightly translucent and apparently crystalline substance. The increase of the muscular coat is uniform, whereas that of the sub-mucous cellular tissue is commonly irregular, and we thus see lobulated protuberances formed on the inner surface of the stomach. Fibrous cancer is the one most easily and most frequently confounded with hypertrophy of the gastric coats. The distinguishing signs are, the preponderating increase of substance in the sub-mucous cellular tissue, and its want of uniformity, the accompanying cartilaginous hardness and closeness of texture, the fusion with the mucous and muscular coats, and particularly the alteration in the muscular tissue just described."

If Rokitansky had given an account of the microscopic appearance of the altered muscular coat, so that the nature of the change might be more apparent, and its existence more easily recognised, we think the sign he dwells on would have had much more value. As it is, we do not see that it can aid us much in a case of doubtful cancerousness. In one instance which we have examined, where doubt might have existed as to the nature of the disease, the pylorus being surrounded by a ring of thickened and altered tissue half an inch wide, and the muscular coat being considerably thickened throughout, we were determined as to the actual existence of cancer; (1) by finding cell substance enclosed in loculi bounded by fibroid structure in the ring surrounding the pylorus; (2) by the absence of the muscular coat at the pylorus, it being apparently converted into the new tissue; (3) by the mucous membrane being in a state of sloughy ulceration over the pyloric ring, or rather the surface of the latter structure being itself exposed and disintegrating. The fact can never be too much borne in mind, at least in pathological studies, that natural groups or families are never exactly defined, so that one is completely separated from the others, but that they are marked by typical instances which hold a central position, while all the intervening space is filled up with gradations of infinite variety. Believing this, it seems quite reasonable to us that from simple chronic thickening of the fibrous tissues, we should advance to fibrous tumours, and from these again to scirrhus by an almost infinite series of intermediate instances. The characters of the types of the different classes are well marked and distinct, but in the transitional forms they are softened down and blended with one another, so that it may be almost or quite impossible to say to which class a given specimen most nearly approximates. Thus, in Dr. Bennett's 22nd Observation, there are some circumstances which seem to indicate a general tendency to the deposition of fibre-forming blastema in various parts, a condition such as one would suppose to be

connected with an unhealthy state of the blood; but again, the extreme thickening of the coats of the stomach, especially at the pylorus, is much more like the condition of growth and independent development which is characteristic of new formations, as tumours. In our case of pyloric cancer just mentioned, there was granular disease of the kidney, very considerable fibroid thickening of the capsule of the spleen, and thickening of the capsule of the liver, especially marked along the superficial portal canals, while no cancer existed anywhere else. These circumstances made us at first suppose that it was simple fibroid formation, not cancerous.

With respect to the question, how far the thickening of the sub-mucous tissue is necessarily the result of inflammation, we think Andral's statements and Dr. Bennett's Observations, particularly the 22nd, afford very weighty evidence in favour of the view that inflammation is not to be regarded as the sole and essential cause. Chronic gastritis, or gastric catarrh, may occur without inducing such thickening, and in some cases where this has been extreme, there has been no history or sign of inflammation. It may be remarked also, that in the numerous cases where thickening has been found coincident with inflammation or irritation of the mucous coat, there is no reason for believing that the sub-mucous tissue itself was in a state of inflammation; blood flowed, no doubt, more abundantly into its nutrient capillaries, in consequence of the active determination to the mucous membrane, and in consequence of this unusual supply the tissue became hypertrophied.* This seems the only explanation of the recurrence of thickening of the wall of the intestine in those cases where there has been no inflammation, or only of brief duration, but a chronic diarrhœa. We do not know exactly how to account for it, but it does certainly appear to us that chronic thickening of the coats of the stomach or intestines must be of much rarer occurrence among us than among our continental brethren. While we meet commonly with similar affections of other parts, while cirrhotic change in the liver is an ordinary event, we scarcely ever have seen an instance of considerable thickening of the fibrous tissue subjacent to the gastro-intestinal mucous membrane. There can, however, be no lack of chronic inflammatory affections of the stomach and intestines, which certainly must be regarded as conditions powerfully predisposing to this change.

Handfield Jones.

(To be concluded in the next number.)

Review IV.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXVI.—*London,* 1853. pp. 496.

The present volume of the 'Medico-Chirurgical Transactions' contains some very valuable papers. One or two of these, on Phlegmasia Dolens, we have made the subject of a separate review; the others we proceed now to notice.

* Cruveilhier says, speaking of a somewhat similar case, "the thickening of the fibrous tunic of the stomach is a consequence of the long-continued fluxion on the mucous membrane."

I. *A Comparative View of some of the more Important Points of the Pathology of Rheumatic and Non-Rheumatic Pericarditis.* By E. L. Ormerod, M.D.

The literature of cardiac disease has been remarkably enriched by various papers in the 'Medico-Chirurgical Transactions.' Dr. John Taylor, Dr. Burrows, Dr. Barclay, and the present author, have especially contributed to advance our knowledge of this subject. In the paper before us, Dr. Ormerod goes over somewhat the same ground as Dr. Taylor, and comes to very much the same conclusions. The following table gives the chief results of the paper:

Out of 1410 cases of all kinds of disease—

1249 were non-rheumatic.
161 were rheumatic.

85 cases were pericarditic { 24 in the 1249 non-rheumatic cases.
61 in the 161 rheumatic cases.

In the 61 cases of rheumatic pericarditis, 24 had also pulmonary complication: viz., pleurisy in 4 cases; pneumonia in 17; pleuro-pneumonia in 3. The pulmonary complication usually followed the pericarditis, and was the result of their common cause. The mortality was 18 per cent., and this was chiefly owing to the pulmonary complication. The average age of the patients was 21 years.

In the 24 non-rheumatic cases, in no less than 13 the disease was not diagnosed; the mortality was 91·6 per cent. The causes were especially pulmonary inflammation (which, of course, preceded the pericarditis, instead of following, as in the rheumatic cases) and renal disease.

7 ensued on inflammation of the lungs, or pleura.
2 „ malignant disease of the pericardium.
1 „ old cardiac disease.
6 coincided with granular disease of the kidney.
4 „ hæmorrhage, or exhaustion.
2 „ scarlatina and erysipelas, respectively.
2 inexplicable.
—
24

The average age was 42 years.

These facts are confirmatory, in great measure, of Dr. Taylor's researches, except that the proportion of renal cases is smaller, and that of antecedent pulmonary inflammation is larger.

II. *On the Development of Torulæ in the Urine, &c.* By Arthur Hassall, M.D. Lond., M.R.C.P.

This paper is remarkable for the extremely careful manner in which the inquiry has been worked out, and for the definite results arrived at. Dr. Hassall, after alluding to the discordant opinions held by writers as to the nature and import of torulæ in the urine, details a host of observations which show that two fungi occur in the urine.

1. The penicilium glaucum, which forms in acid urine, without sugar, and with or without albumen. It is destroyed by alkalinity, and its

rapidity and amount of development appeared to be in direct proportion to the acidity. It was found in 24 out of 32 urines submitted to examination. Beautiful plates of the plant in its stages of sporules, thallus, and aërial fructification, are given. We may observe that the rapid formation of this fungus in acid non-saccharine urine was described by Dr. Spencer Thomson some years ago.*

2. The sugar fungus forms only in diabetic urine, and even in cases in which the quantity of sugar is extremely small, it is distinct from the penicilium glaucum, and can be easily distinguished by the microscope. It requires acidity and free exposure to air, and will not appear, even in saccharine urine, if these conditions are excluded.

The practical importance of this paper lies in the fact, that the error of concluding the presence of sugar from the existence of the penicilium is pointed out, and that the actual existence of sugar may be proved from the appearance of the sugar fungus, even when the copper test gives no evidence.

Most beautiful plates are given of the sugar fungus, and by their means any confusion between the sporules, thallus, and fructification of the one and other plant, can be avoided. It must be remembered, however, that the two fungi can coexist.

III. *Sequel to a Case of Albuminous and Fatty Urine, published in the 'Medico-Chirurgical Transactions' for* 1850, *with some account of Two other Cases of so-called Chylous Urine.* By H. Bence Jones, M.D., F.R.S.

In the interesting case of chylous urine recorded in the paper referred to, a most remarkable cure seemed to be effected by the use of gallic acid. This cure, however, was not permanent; on the re-appearance of the symptoms, gallic acid was again resorted to, but with less benefit. The following table gives the particulars at a glance:

"After the original attack had existed more than 10 months, the

	Days.
Urine was found to be chylous for	101
It then became healthy, after gallic acid, for	232
Then, in the 1st relapse, the urine was chylous for	2
It then became healthy, after gallic acid, for	80
Then, in the 2nd relapse, the urine was 6 days clear, chylous for	15
It then became healthy, after gallic acid, for	237
Then, in the 3rd relapse, the urine was chylous for	2
It then became healthy, after no gallic acid, for	16
Then, in the 4th relapse, the urine was 1 day clear, chylous for	125
It then became healthy, after gallic acid, for	534

During and after the original attack the quantity of

gallic acid taken was about			5 ounces		
,, 1st relapse	,,	,,		7 drachms.
,, 2nd relapse	,,	,,	3 ,,	5½ ,,
,, 3rd relapse	,,	,,	none.	
,, 4th relapse	,,	,,	30 ,,	3 ,,

Total amount of gallic acid taken, nearly 40 ounces." (p. 90.)

* Medical Times and Gazette, 1850, vol. xxi, p. 184.

The gallic acid is, therefore, Dr. Jones remarks, no specific, though its beneficial action as an astringent is obvious. Tannic acid was used, but caused more nausea and headache than the gallic acid.

In the second case, the gallic acid evidently was of little use, but tannic acid and alum were of greater benefit. The disease, as usual, was greatly augmented by fatigue.

The third case, in which only one dose of gallic acid was taken, is recorded to show the length of time the disease can last. The first appearance of the chylous urine was in 1827; with various remissions, it was still chylous in 1853, when the last record of the case was made. Out of these 26 years, the urine had been chylous for 13 years. From 1846 it has been constantly chylous. In 1851 the patient (a lady) was aged 57, and weighed 14 stone. There was pain over the kidneys, but otherwise the health was good.

IV. *On Degeneration of the Placenta at the End of Pregnancy.* By Robert Druitt, M.R.C.P.

We have elsewhere referred to this important paper.

V. VI. VII. *Cases of Hypertrophy of the Tongue.* By G. M. Humphry, Esq., J. Hodgson, Esq., and T. P. Teale, Esq.

Mr. Humphry relates a very interesting case of hypertrophy of the tongue cured by operation, notices others which have occurred, and sums up the literature of the subject. The following extract embodies the principal points of interest:

"From the cases above mentioned, and those described by other writers, all of which present great similarity in their general features, we learn that the disease is one of sufficiently definite character to merit special notice. Though it sometimes originates in an inflammatory affection, it often appears to be independent of any such cause. It commences usually in childhood or early infancy, or is congenital. If it be early attended to, it admits of an easy cure by compression. If it be allowed to take its course, the prolapsing organ increases; at first, in greater proportion than the growth of the child. After a time it would appear to become stationary, or to grow only with the growth of the body, for in none of the cases, even those in which the deformity had existed for many years, had the tongue attained to greater size than one's fist. . . . . Spontaneous cure rarely, if ever, takes place after the prolapse has been established. The operation of removal is not attended with much danger, and the remaining portion of the tongue shows no disposition to enlarge again." (p. 126.)

Mr. Humphry has collected altogether 25 cases; 6 of which were treated by pressure, 7 by ligature, and 12 by amputation. In one case of ligature and one of amputation, the results were fatal; in all, the disease was cured.

To these cases, Mr. Hodgson supplies another, successfully treated by ligature; and Mr. Teale another, successfully treated by pressure.

VIII. *Case of Popliteal Aneurism treated by Compression.* By J. Monro, M.D., Battalion Surgeon, Coldstream Guards.

In this case, moderate pressure was made on the femoral artery below Poupart's ligament, by means of an apparatus. As this gave rise to great

uneasiness, it was removed, and the artery, below the origin of the profunda, was compressed by the hand, a succession of assistants being provided, for eight hours in each day. Three days afterwards, the pressure was reapplied, the point of pressure being changed from time to time. In seventeen days the sac ceased to pulsate; in about an hour, however, pulsation began to return, and the instrument was reapplied for two days. The patient was kept in hospital for some time, but was at length discharged. He was re-admitted in seventeen days, with evident signs of aneurism of the abdominal aorta, of which he soon after died. On dissection, the femoral and popliteal arteries were of the natural size until opposite the centre of the popliteal space; the sac was obliterated, and so was the artery below it as far as the subdivision into the anterior and posterior tibial arteries. The profunda and all its branches were enlarged; the anterior tibial was as large as the posterior, which latter was of its natural size. The peronæal, the muscular branches of the semi-membranosus, vastus externus, and biceps, were all large, and so were various other vessels, for a full account of which we refer to the paper. The femoral vein was pervious; the popliteal was obliterated for the space of three inches by the pressure of the sac.

The chief point of interest in this case was the very moderate pressure sufficient to cause obliteration of the sac.

IX. *A further Account of Fatty Degeneration of the Placenta.* By Robert Barnes, M.D.

The review by Dr. Barnes, in our present number, on placental diseases, renders it unnecessary for us to analyze this paper.

X. *On Phlegmasia Dolens.* By F. W. Mackenzie, M.D., London.

We have reviewed this paper elsewhere.

XI. *On some Points in the Pathology of Yellow Fever.* By Croker Pennell, M.B. Lond., Physician to the Livramento Hospital, Rio de Janeiro.

It is well known that many patients in yellow fever die very suddenly, and at a time when the severity of the general symptoms had caused no apprehensions of so rapidly fatal a termination. Dr. Pennell, being informed by a French physician, Dr. Lacaille, that in every case in which this occurred, large fibrinous clots were found in the heart and large vessels, has paid especial attention to the formation of ante-mortem clots, and affirms that, in 50 autopsies, clots were found in every case except those in which depletion had been largely employed. He assigns the following reasons for supposing that these clots had formed before death:—1. They were found quite "bloodless and perfect," within an hour after death; 2. A peculiar rumbling or churning sound replaces, for some hours before death, the natural cardiac sounds; 3. Fibrinous clots are found in other organs; 4. Without the supposition that these clots form during life, the phenomena are inexplicable.

With respect to these arguments, we may at once dismiss the fourth, as begging the question; the first argument is not so convincing as Dr. Pennell supposes, since these decolorized clots do certainly form, in some cases, very rapidly after death. The alterations in the cardiac sounds are not detailed with much precision, and we should hesitate to admit the ante-mortem production of clots on this evidence alone. With respect to the third argument, the "other organs" turn out to be merely the ureters, which in two cases were found blocked up with fibrinous coagula. This is an interesting observation, but we do not see how the formation of coagula in the pelvis and ureters, during the hæmorrhage from the kidneys (which, we presume, occurred in these two cases), can be held to justify any argument as to the cardiac clots.

XII. *On the Treatment of Obstinate Strictures of the Urethra by External Incision upon a Grooved Director.* By James Syme, Esq., F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh.

We reserve this valuable paper for future consideration.

XIII. *Further Researches on the Pathology of Phlegmasia Dolens.* By Robert Lee, M.D., F.R.S.

This paper has been elsewhere reviewed.

XIV. *On the Use of Two Needles at once in certain Operations on the Eye, especially in those for Capsular Cataract and Artificial Pupil.* By William Bowman, Esq., F.R.S.

The use of two needles at once in certain operations on tough capsular and membranous cataracts, will probably turn out to be one of the most important improvements lately introduced into the operative part of ophthalmology. For a description of the instruments and of the operation, we must refer to the paper itself.

XV. *Analysis of the Cases of Injuries of the Head, examined after Death in St. George's Hospital, from January,* 1841, *to January,* 1851. By Prescott Hewett, Esq., Assistant-Surgeon to St. George's Hospital.

In the present paper, only scalp wounds without fracture, and cases of fracture, are considered. Other injuries to the head are left for a future communication.

1. *Scalp Wounds without Fracture.*

Thirty-three fatal cases occurred in ten years. The total number of scalp wounds admitted during that time is not mentioned. Of the 33 cases, 10 died from causes unconnected with the wound, and 23 from sequences of the wound. These sequences were, diffuse cellular inflammation, erysipelas,

inflammation of the membranes or of the brain, suppuration between the bone and the dura mater, abscess in the brain. The relation of these various lesions is not very clearly given. We quote Mr. Hewett's own words:

"Diffuse cellular inflammation occurred in 17 cases, and in 12 this was accompanied by erysipelas. This inflammation presented itself under the occipito-frontalis muscle, in the shape of simple œdema, of lymph and pus, or of suppuration and sloughing, undermining and, in one instance, completely separating the scalp from the parts below. In 4 cases, there was also diffuse inflammation of the neck, which spread down to the mediastina in 2 cases, and it was accompanied by œdema of the larynx in 2. The diffuse inflammation, running on to sloughing, was in 2 cases followed by hæmorrhage, which in 1 was of a very severe character.

"In 12 cases, no traces whatsoever of inflammation were found, either in the membranes or in the brain; of these, the diffuse inflammation proved fatal in 3; and in the other 9, death was caused by purulent infection: in addition to the injury of the head, there was in 1 of these simple fracture of the internal malleolus, and in 1 there was fracture of several ribs.

"In 10 of the remaining 11 cases, inflammation existed about the membranes or the brain, in 8 of which suppuration, more or less extensive, was found between the bone and the dura-mater; 5 of these were complicated with purulent infection. In addition to the injury of the head, there was also in 1, compound fracture of the leg; and in 1, fracture of the spine, but this was not accompanied by any symptoms.

"In 1 case, sloughing of the dura-mater, without any injury to the internal part of the bone, had taken place opposite to a large abscess, which originally had begun in the structure of the brain.

"In all these cases, the bone was exposed; in 4, the exposed bone was discoloured, being of a dark, or of a yellow tinge; in 5, matter was detected in the diploë. The trephine had been applied in 3 of the cases of suppuration between the bone and the dura-mater, as well as in that of the abscess of the brain.

"These 23 patients were, for the most part, persons of dissolute habits, and many were addicted to hard drinking." (pp. 322, 23.)

The suppuration between the bone and the dura-mater was generally circumscribed; in the parietal region, however, it followed the branches of the middle meningeal artery, and reached even the foramen spinosum in 3 out of 8 cases. Contrary to the experience of Mr. Pott, the suppuration was never found to be confined to the outer surface of the dura-mater. The trephine was applied in 3 cases. Mr. Hewett mentions that, under such circumstances, he has never known the application of the trephine to be successful.

2. *Fractures and Separation of the Sutures.*

In the ten years, 78 cases of fracture of the skull were examined; 56 were simple, and 22 were compound; extreme separation of the sutures co-existed in 14 cases. In the 56 cases of simple fracture, there was only one instance in which the injury was confined to the spot struck. In the 22 cases of compound fracture, in no less than 9 the injury to the bones was strictly limited to the original seat of the injury.

With respect to the direction of fractures, the following remarks are interesting:

"Practically, the skull may be divided into three different zones or segments; an anterior zone, formed by the frontal, the upper part of the ethmoid, and the

fronto-sphenoid; a middle zone, formed by the parietals, the squamous, and the anterior surface of the petrous portions of the temporals, with the greater portion of the basi-sphenoid; and a posterior zone, which is formed by the occipital, the mastoid, and the posterior surface of the petrous portions of the temporals, with a small part of the body of the sphenoid.

"Fractures of the skull, beginning at the seat of the blow, and from thence spreading into the base, are oftentimes, it will be found, limited exactly to the bones belonging to each of these three different zones." (p. 338.)

Bleeding from the ear, in severe injuries of the head, is a valuable diagnostic sign of fracture of the base, implicating the petrous portion of the temporal bone. The discharge of watery fluid from the ear occurred in 3 cases out of the whole number. Two additional cases are referred to as having lately been seen by Mr. Hewett. The age of these five persons was respectively 42, 46, 43, 63, and 52; so that this discharge of watery fluid would not, then, appear to be more common in young persons than in adults, as is often asserted.

Mr. Hewett has no doubt that in most cases, but not in all, this fluid is the cerebro-spinal fluid.

We strongly recommend the perusal of this very practical paper, which our limits prevent us from considering more at length.

XVI. *A Case of Perforating Ulcer of the Œsophagus, which caused Death by Penetrating the Aorta.* By W. H. Flower, Esq., Curator to the Middlesex Hospital Museum.

An extremely good description of a most uncommon case, the nature of which is indicated by the title of the paper. The ulcer was seated about 3 inches above the cardiac orifice; the œsophagus and aorta were only loosely connected; the distance between the lining membranes of each tube was 7 lines. The patient died from profuse hæmorrhage; the blood was partly vomited, partly filled the stomach and the duodenum.

XVII. *On Small-Pox and Vaccination. Analytical examination of all the cases admitted during 16 years at the Small-Pox and Vaccination Hospital, London.* By J. F. Marson, Esq.

It is extremely difficult to give an accurate account of this elaborate paper without inserting the tables. We must content ourselves, then, with a few extracts. The average annual mortality in the Small-Pox Hospital has been 21 per cent.; the range being from 15 to 29 per cent. Out of the whole number (5797) of small-pox patients in the 16 years, 47 cases were after a previous attack of small-pox, or inoculation (1 per cent.); 3094 cases (53 per cent.) were after vaccination. The mortality among the various classes was as follows:

In 2654 unprotected cases, 35½ per cent. died.
In 14 cases protected by previous small-pox, none died.
In 27 cases protected by inoculation, 23 per cent. died.
In 2787 cases protected by vaccination, 5¼ per cent. died.
In 290 cases said to have been vaccinated, but who were without cicatrices, 21¾ per cent. died.

It is worthy of notice, that among those who had been vaccinated,

but bore no cicatrices, the mortality was much greater than among those whose arms showed good cicatrices.

Natural small-pox is most fatal in infancy and in advanced life: under 5 years it is 50 per cent.; from 5 to 20 years it is considerably less; it increases after 20 years, and after 30 exceeds the mortality of infancy; after 60 years scarcely any escape.

Dr. Marson believes that vaccination in in fancy is protective to puberty against the fatality of small-pox; how far it is protective after this time is not very easy to say, but it appears that among those who have been vaccinated with great care, the mortality from subsequent small-pox never exceeds 1 per cent.

XVIII. *Observations on the State of the Blood and the Blood-vessels in Inflammation.* By T. Wharton Jones, F.R.S.

We reserve this interesting paper for separate consideration.

XIX. *On Intermitting Diabetes, and on the Diabetes of Old Age.* By H. Bence Jones, M.D., F.R.S.

Seven cases of diabetes are related, in which the attacks of diabetes were transitory and recurrent. Immediately before and after the sugar disappeared, there was an excess of urea; free uric acid and oxalate of lime appeared in the urine. This seems to indicate, the author thinks, "the passage of diabetic indigestion into acid indigestion."

Several cases of diabetes in persons aged over 70 are related, though there is nothing that calls for special remark. At the end of the paper Dr. Jones observes that, from some experiments not yet published, he finds that porter contains from 20 to 40 grains of sugar in every ounce; ale from 12 to 130 grains; port wine contains a good deal of sugar; sherry a less quantity, and claret none at all.

XX. *An Account of a Dissection of an Ovarian Cyst, which contained Brain.* By Henry Gray, F.R.S.

In this singular case an ovarian cyst contained a brain-like substance, which, on microscopic examination, was found to present all the characters of true brain; there were nerve tubules, nuclei, and nucleated vesicles.

XXI. *An Account of an Instance of Remarkable Deformity of the Lower Limbs.* By George Viner Ellis, Esq.

This is a careful dissection of the body of a man who was exhibited in London some years ago, and who, in spite of unusual deformity, was possessed of extraordinary strength. The chief peculiarity was the almost entire absence of one femur, and the consequent changes in the muscles and in the course of the femoral artery.

XXII. *Observations on Cystic Disease of the Testicle.* By T. B. Curling, F.R.S.

Mr. Curling thus sums up the facts of his paper:

"1. Cystic disease of the testicle occurs in two forms—an innocent and a malignant.

"2. Both forms are the result of morbid changes in the ducts of the rete testis, this part of the gland being the sole seat of the disease.

"3. The innocent form of the cystic disease is characterised by the presence of tesselated epithelium in the cysts.

"4. The malignant form is characterised by the presence of nucleated cancer cells in the cysts.

"5. Enchondroma occurs in both forms of the cystic disease, and almost constantly in old cases of the innocent, the cartilage being developed within dilated tubes." (p. 457.)

We should doubt whether the diagnosis between the two forms can be made thus easily by the microscopic characters alone.

XXIII. *Additional Experiments on the Excitability of Paralyzed and Healthy Limbs by the Galvanic Current.* By R. B. Todd, M.D., F.R.S.

Fourteen cases of hemiplegia are related, in which very careful experiments were made on the excitability of the paralyzed, as compared with the non-paralyzed, muscles. The result was, that the excitability of the paralyzed muscles was impaired in all but three cases: in two of these the greater excitability of the paralyzed muscles was manifested only by the inverse current. In each of these 3 cases the paralyzing lesion was "more or less of an irritative kind."

The results were the same, whether the trough or the coil machine was used.

Dr. Todd believes, and with apparent justice, that these experiments, added to those of Duchenne and Brown-Sequard, confirm the inferences drawn in his former paper on this subject, and are opposed to the statements of Dr. Marshall Hall, that the excitability of paralyzed muscles varies according to the seat of the lesion, being increased when the influence of the brain is cut off, and the spinal cord is uninjured, and being diminished when the cord itself is diseased.

REVIEW V.

1. *Researches on the Pathology of Obstructive Phlebitis, and the Nature and Causes of Phlegmasia Dolens.* By F. W. MACKENZIE, M.D., Fellow of University College, &c. ('Medico-Chirurgical Transactions,' vol. xxxvi.)

2. *Further Researches on the Pathology of Phlegmasia Dolens.* By ROBERT LEE, M.D., F.R.S., &c. ('Medico-Chirurgical Transactions,' vol. xxxvi.)

THERE is no merit in what are called simple views of disease, its causes and remedies—for the plain reason, that they are not true. Neither in health nor disease is "the paragon of animals" a simple machine, in the

ordinary sense of the word. It is true, that the means employed are admirably and economically adapted to the end to be attained; that there is no superfluous complexity, no needless combinations; but still, as a whole, the functions of the human body are complex, interwoven, and interdependent. So it is in disease: causes act and react, are combined and multiplied; apparently simple effects may result from complex causes, compound effects from simple causes; the interdependence of organs involves their mutual suffering, and even the organ primarily affected may have its distress increased by reflex action from other organs it has secondarily disturbed. Thus it is that any attempt at a simple solution of these varied phenomena must necessarily fail: such theories carry within them the cause of their own destruction. The non-recognition of the complex phenomena of disease, gave their erroneous character to the successive systems of solidists, humoralists, to the mechanical and chemical schools, the school of Broussais, &c.; each contained a portion of truth, but became an error because of the portion it omitted. Modern eclecticism, it is to be hoped, will avoid this one-sided blunder, yet it is very necessary that we should be reminded of it occasionally, so as to be continually on our guard.

We have been led into this train of thought by the perusal of the two valuable papers placed at the head of this article, written upon a disease which has given rise to very different, if not opposite views, and which is not altogether explicable by any single theory hitherto broached.

It is unnecessary to enter upon a description of the disease which has been called Phlegmasia Dolens, as it is, doubtless, sufficiently well known to our readers; we shall confine ourselves, for the brief space allotted to us, to a consideration of its pathology, which is of comparatively modern origin. We may dismiss at once the older belief, that it arises from a metastasis of the milk or lochia, as being a mere fancy; and even the more feasible proposition of its being inflammation of the cellular tissue, the lymphatics, or the entire structures of the limb, as being guesses rather than the result of pathological inquiry. The credit of the first step towards a discovery of its true nature is due to the late Dr. Davis, professor of midwifery in University College, London, who, in 1817, examined the condition of the veins of the limb in a patient who had died of this disease. He found—

"The femoral vein from the ham upwards, the external iliac and common iliac veins as far as the junction of the latter with the corresponding trunk of the right side, were distended, and firmly plugged with what appeared, externally, a coagulum of blood. The femoral portion of the vein, slightly thickened in its coats, and of a deep red colour, was filled with a firm, bloody coagulum, adhering to the sides of the tube, so that it could not be drawn out. As the red colour of the veins might have been caused by the red clot everywhere in close contact with it, it cannot be deemed a proof of inflammation. The trunk of the profunda was distended in the same way as that of the femoral vein, but the saphena and its branches were empty and healthy. The substance filling the external iliac and common iliac portions of the vein was like the laminated coagulum of an aneurismal sac, at least with a very slight mixture of red particles: the tube was completely obstructed by this matter, more intimately connected to its surface than in the femoral vein—adhering, indeed, as firmly as the coagulum does to any part of an old aneurismal sac, but in its centre there was a cavity containing about a tea-

spoonful of a thick fluid, of the consistence of pus, of a lightish-brown tint, and a pultaceous appearance. The uterus, which had contracted to the usual degree at such a distance of time from delivery, its appendages and bloodvessels, and the vagina, were in a perfectly natural state. There was not the slightest appearance of vascular congestion about the organ, nor the slightest distension of any of its vessels. Its whole substance was, on the contrary, pale, and the vessels everywhere contracted and empty."*

From this period, Dr. Davis taught that phlegmasia dolens was owing to inflammation of the crural veins, although his paper was not published until some time afterwards. In France, MM. Bouillaud and Velpeau entered upon the investigation; and, in 1823, the former published cases with dissections, to show that the crural veins were obliterated in women who suffered from this disease after delivery. In 1824, the latter in his paper, included the symphyses, the veins, and the lymphatics, as forming the seat of the disease. In 1826, Mr. Guthrie threw out a suggestion, that perhaps the crural phlebitis might be merely an extension of inflammation from the veins of the uterus; and in 1829, the co-existence of uterine and crural phlebitis in puerperal phlegmasia dolens was demonstrated by the careful and minute observation of Dr. Robert Lee. He traced the diseased crural veins upwards to the uterus, and found the veins of that organ equally diseased, and this not in one case only, but in several; from which he inferred—

"That inflammation of the iliac and femoral veins gives rise to all the phenomena of that disease; and that, in phlegmasia dolens, the inflammation commences in the uterine branches of the hypogastric veins, and subsequently extends from them into the iliac and femoral trunks of the affected side;"—

And further, that this propagation by extension is an essential characteristic of the disease; for in commenting upon two cases of Mr. Wilson's, in which the veins of the uterus contained pus, he observes:

"As none of the symptoms of phlegmasia dolens were present in either of these cases, and as neither pain nor swelling occurred in the left inferior extremity of the patient whose case I first detailed, though the common and internal iliac veins were both completely impervious, it would seem to follow, that it is essentially requisite that the inflammation should extend from the iliac into the principal veins of the extremity."†

The paper by Dr. R. Lee, under our notice at present, is, in fact, a continuation of his former one, and contains "the results of the last twenty-four years' experience." It is in all respects a most valuable contribution to our knowledge of the disease, and such as few but Dr. Lee could have supplied. In addition to the 13 cases in his former papers, the author has given 43 cases of phlegmasia dolens: in 9, the post-mortem examination is described; in 20, the history of the case is given, but as the patient recovered, further investigation was impossible; 9 other cases are detailed in which phlegmasia dolens was unconnected with pregnancy or parturition, but in which inflammation of the uterine veins co-existed; and in 5 other cases, the inflammation of the veins of the lower extremities resulted from injury or disease. Nothing can be more definite than the establishment of the fact of the co-existence of crural and uterine

* Letter from W. Lawrence, Esq., in Davis' Obstetric Medicine, vol. ii. p. 1024.

† Medico-Chirurgical Transactions, vol. xv. part 2, p. 393.

phlebitis in those puerperal cases, where a post-mortem examination was permitted; and Dr. R. Lee concludes, that

"The cases and dissections of the distinguished pathologists previously referred to, and those contained in this and my previous communications to the Society on phlegmasia dolens, prove, in the most conclusive manner, that inflammation of the iliac and femoral veins is the proximate cause of the disease; and that, in puerperal women, the inflammation commences in the uterine branches of the hypogastric veins. It has likewise been demonstrated by morbid anatomy, that phlegmasia dolens is a disease which may take place in women who have never been pregnant, and in the male sex, and that, under all circumstances, the proximate cause is the same."*

In the present day, when the pathology of the fluids, and especially of the blood, is exciting so much attention, it could not be expected that this view of the disease would pass unquestioned, particularly when it is considered that this explanation, applied to a certain class of cases, is at least incomplete, and of another, it is no explanation at all; accordingly, Dr. Mackenzie has entered the field with great zeal and ability, and, by an elaborate series of experiments, has endeavoured to prove that phlegmasia dolens is a blood-disease, and that the affection of the veins is "of secondary importance, or an effect rather than a cause of disease."

We shall lay before our readers a very brief account of these experiments, or, rather, of the conclusions deduced from them by the author. The first question Dr. Mackenzie attempted to decide was, whether phlegmasia dolens could be produced by artificial irritation or injury of the veins, by ligature, by chemical or mechanical irritation of the lining membrane, or by compression. 1. A ligature was applied to the iliac vein of dogs which were killed at intervals of twenty-four, seventy-two, and ninety-six hours, or nine days afterwards. Evidences of inflammation of the veins were found at the seat of the ligature, but limited to its immediate neighbourhood, and more marked in those dogs which had been spared the longest after the operation; whereas the general symptoms, which were slight and transient, were best developed immediately after the ligature had been applied. The swelling of the limb was soft and inelastic, without tenderness on pressure. 2. The iliac veins were irritated by a strong solution of nitrate of silver, applied upon lint, or by a bougie introduced and maintained there for several days: with the former, there was little constitutional disturbance, the leg was swollen and inelastic, but could be moved; these symptoms soon began to subside, and the dogs appeared quite well. On dissection, ten days afterwards, the external and common iliacs were filled with a fine coagulum, but which was confined to the irritated portion of the vein. 3. A metallic compress, applied to the right femoral vein of a dog for six hours, gave rise to neither swelling nor loss of power; and at the compressed part only, the coats of the vein were thickened, indurated, and opaque.

"Then, as the result of obstruction, inflammation, ulceration, and total division of the common iliac vein, produced by the application of the ligature, we fail to observe more than the necessary effects of mechanical and temporary obstruction of the vessel and of inflammation of it, limited to the immediate seat of ligature. There is an absence of any constitutional fever or disturbance; the swelling of the

* Medico-Chirurgical Transactions, vol. xxxvi. p. 311.

limb is neither elastic nor abiding, but simply oedematous, and there is no impairment of either its sensory or motor functions." (p. 181.)

The author next proceeds to determine how far irritation or injury of the external coats of veins is connected with the causation of obstructive phlebitis. Ligatures, chemical irritants, and mechanical contusion, were the tests employed, and the following are Dr. Mackenzie's conclusions:

"1. That obstruction of veins is not a necessary consequence of all forms of venous inflammation, and, in particular, of that which follows upon irritation or injury of their external coat.

"2. That inflammation of veins thus excited is not disposed in a healthy animal to extend itself indefinitely, but, on the other hand, is strictly limited to the immediate seat of such irritation or injury.

"3. That the external coat of veins very readily reacts under the influence of irritating causes; this being, in these cases, morbidly vascular, covered with inflammatory lymph, and adherent to the surrounding tissues.

"4. That such reaction, followed by considerable inflammation of the external coat, may occur without giving rise to any corresponding inflammation of the lining membrane: for, in these cases, the latter was healthy, and the vein consequently pervious, or at least free from any inflammatory exudation." (pp. 186, 187.)

We now arrive at a very important question: suppose an irritant be introduced into a vein, are the stagnation and coagulation which result, to be attributed to the effusion of coagulable lymph by the inflamed vein; to the action of the inflamed vein upon the blood; or to the action of the irritant primarily, upon the blood, and secondarily upon the vein, inducing phlebitis? To determine this point, a portion of the vein, emptied of blood, was isolated by two ligatures, and an irritant applied to it, and then removed, and the current of blood allowed to flow through the irritated vessel. The results of the experiment were very decisive: coagulable lymph was not poured out, but the blood was speedily coagulated to an extent commensurate with the irritation, and that not from any injury of the vessel. The coagulation must have been the result of some impression produced upon the blood by the lining membrane thus irritated, and not from any independent action on that fluid, inasmuch as it was excluded during the application of the irritant. All the phenomena of obstructive phlebitis appear to have been produced in the coats of the veins, and in the coagula contained within them, but restricted to the space occupied by the irritation, the blood and veins beyond having been found healthy.

As it has been stated that coagulation might take place spontaneously in the vessels from extreme exhaustion, physical or vital, or be caused by the direct action of pus or other morbid matters upon the blood, Dr. Mackenzie made a series of experiments to decide each point. Dogs were repeatedly bled and kept low, but without any coagulation taking place; and even when a ligature or compression was added, the coagulation was limited to the seat of injury, so that we may dismiss this from the category of causes.

Dr. Mackenzie has arrived at a different conclusion from that of Mr. Henry Lee as to the effect of pus in promoting coagulation of the blood, or rather, he adopts a different explanation of the coagulation so produced; but into this question we shall not enter at present.

So far, then, as we have gone into these experiments,

"It would thus appear that whilst phlebitis may occur in a variety of forms without giving rise to extensive obstruction of veins—whilst such obstruction is not producible by mechanical injury of these vessels, by irritation of their external coat, by actions primarily taking place in the blood, and does not arise from the effusion of lymph into their interior—it is yet readily produced by irritation of their lining membrane; that the obstructing material is not lymph, but blood; and that this is immediately coagulated in the vein by some impression made upon it by its lining membrane when thus morbidly irritated or excited." (p. 203.)

Further experiments seemed to prove, also, that extensive obstruction of veins may be excited by the passage of irritating fluids over their lining membrane, without actual inflammation being excited, from which Dr. Mackenzie concludes that phlebitis is not a primary condition of venous obstruction, but rather a consecutive phenomenon.

Having thus settled what we may call the preliminary questions, the author, in the last place, proceeds to inquire how far the irritation of the lining membrane of the veins, which gives rise to the obstruction, and which may occur without actual injury of these vessels, may be produced by a general vitiation of the blood alone. An experiment of great interest is detailed: the *right* femoral vein was ligatured on June 16th; at first there was some little œdema, which quite disappeared in seventy-two hours, and the limb appeared perfectly natural. The *left* femoral vein was then ligatured, and above the ligature one drachm and a half of lactic acid, diluted with ten drachms and a half of tepid water, was injected towards the heart. In twenty-four hours the corresponding extremity (the left) was greatly swollen throughout, its temperature was raised, it was painful on pressure, and the dog moved it with difficulty. It continued in much the same state, until it was killed.

"The iliac veins of the left side were obstructed with a firm coagulum, which extended somewhat into the cava, *whilst the femoral and all the principal veins of this extremity were found similarly obstructed.* On opening these vessels it was found that the external and common iliac veins were filled with a firm coagulum, which was throughout adherent to their lining membrane, and, on removing it, some red gelatinous-looking matter was left upon the interior of the veins, which, for the most part, could be readily brushed away, but here and there portions of it were firmly adherent. The colour of the lining membrane of these veins was somewhat redder than natural, but the membrane itself was, for the most part, smooth and polished. At its junction with the femoral, the coats of the external iliac vein were thickened and opaque, and it did not collapse on being cut through. The left femoral vein was filled with a dark coagulum, which was somewhat soft and grumous immediately below the ligature, but which elsewhere was of firm consistence. It was slightly adherent, and, on removing it, a little gelatinous-looking matter was left upon the lining membrane, which could be easily brushed away. This vessel was neither thickened nor opaque, nor was any increased vascularity of its coats observable. The principal branches of the femoral vein were all filled with firm coagula, but these were very slightly, if at all, adherent to the interior of the veins, ... On making transverse sections of the different coagula found in the veins, it was observed that their exterior was much firmer than their interior. The former consisted of firmly coagulated blood, the latter of little more than a grumous semi-fluid material. Hence it seems reasonable to conclude, that coagulation had commenced at the circumference of the coagula, where the blood was in contact with the lining membrane of the veins." (pp. 207, 208.)

In this case it is worthy of notice, and we shall revert to it by and bye, that the morbid appearances in the iliac and femoral veins may be said to have been continuous, although they could not have been propagated by extension, inasmuch as a ligature interposed. Nor did the morbid condition of the veins depend upon the operative proceedings, for a similar operation, but with the injection of tepid water only, produced no effect whatever. This experiment was repeated in a variety of ways, in different animals, with very similar results:

"In all, considerable obstruction of veins followed the track of the irritant in its progress onwards towards the heart, whilst in the majority there was also obstruction of the distal veins of the extremity, which, consistently with the results of preceding experiments, could only have arisen from the action of the irritant after it had gone the round of the circulation." (p. 209.)

The final conclusions at which Dr. Mackenzie has arrived we shall give at length:

"1. That inflammation of the iliac or femoral veins will not alone give rise to all the phenomena of phlegmasia dolens.

"2. That the extensive obstruction of the veins met with in this disease, is not producible in a state of health by merely local causes, such as injury or inflammation of these vessels.

"3. That irritation of the lining membrane of veins independently of such local injury or inflammation, may, under certain circumstances, give rise to obstruction of these vessels, and this to an extent commensurate with that of the irritation which may have been excited within them.

"4. That such irritation of the lining membrane of veins, giving rise to obstruction, and consecutively to all the phenomena of *obstructive* phlebitis, ... is therefore to be sought for rather in a vitiated condition of the blood, than in any local injury, inflammation, or disease of the veins." (pp. 212, 213.)

We have thus briefly, but we trust correctly, sketched the outline of Dr. Mackenzie's experiments, and have given the inferences he draws from them; we have rather confined ourselves to enumerating the broad principles involved than to investigating their details; and with regard to their analogical value in explaining the phenomena of disease, we must beg our readers to bear in mind that they are subject to two unavoidable but very serious drawbacks. In the first place, the subjects of these experiments were healthy, but the patients in whom phlegmasia dolens occurs are either the subjects of actual disease at the time, or in that condition of body which strongly predisposes to disease; causes, therefore, which might be innocuous, or nearly so, to one class, might be expected to act most injuriously upon the other;—so far, we think, the analogy fails. Secondly, the mechanical irritant, in Dr. Mackenzie's latter experiments, differed unavoidably from the impure constituent of the blood in diseases depending upon deterioration of this fluid: it is not easy to determine how much this circumstance detracts from their analogical value, but certainly a large deduction must be made. It has also been suggested, that such experiments on animals might differ in results from similar experiments on human beings; but of this we do not feel quite sure. But allowing for these drawbacks, we may certainly admit, as proved by these experiments, that in healthy subjects irritation of the veins, so as to excite phlebitis, will not give rise to phlegmasia

dolens, unless, at the same time, the blood be vitiated, primarily or secondarily. At the conclusion of his paper, Dr. Mackenzie has endeavoured to support his views, and in many respects successfully, by a minute and careful analysis of a hundred cases, collected from various sources.

Having entered thus far into the subjects of these two excellent essays, let us now see how the question stands, and whether it is possible to arrive at any definite conclusion.

Dr. R. Lee conceives the disease—phlegmasia dolens—to be, primarily and essentially, phlebitis, which, in puerperal cases, is propagated by extension from the inflamed uterine veins; and he appeals for proof to the uniform results of his dissections, and to the characteristic symptoms of the disease. Dr. Mackenzie, on the other hand, thinks that the disease depends primarily upon a vitiated condition of the blood, which gives rise to venous obstruction, and afterwards to phlebitis, but that the phlebitis is rather consequent upon the obstruction than the cause of it; and he appeals to his experiments for proof, and to his analysis of cases for confirmation. Dr. Lee regards the disease as a local affection; Dr. Mackenzie maintains that it is a constitutional disease.

The fact, however, which we believe dissection has revealed in all the examples of phlegmasia dolens where an autopsy has been obtained, is, the presence both of venous obstruction and evidences of inflammation; and although certain of Dr. Mackenzie's experiments seem to show that coagulation and obstruction may exist without inflammation, yet inasmuch as they were subject to the drawbacks already mentioned, we confess that in our judgment their analogical value completely fails, when placed in contrast with the results of post-mortem examination. We have no hesitation, therefore, in concluding, that phlebitis is an essential feature of the disease, but whether primary or secondary is another question, which we may now examine a little more closely; for which purpose we shall distinguish crural phlebitis in puerperal women from other cases of the disease.

Dr. R. Lee maintains, not only that crural phlebitis is primary and essential, but that it is produced by an extension of inflammation from the veins of the uterus in puerperal phlegmasia dolens. Of the co-existence of uterine and crural phlebitis in Dr. R. Lee's cases there can be no question, for it was proved by dissection: nor do we doubt the co-existence of these two affections in most cases; but this does not decide the question of the local or constitutional origin of the disease, for Dr. Mackenzie's theory will apply to them quite as satisfactorily as that of Dr. Lee's. At first sight, indeed, the continuity of the disease would seem a conclusive argument for its propagation by extension, but upon consideration we shall find many obstacles to this view, many cases to which it will not apply, and some reasons which seem to point to a more general affection as an influential cause.

As a general rule, not without exceptions it is true, we may observe that the course, in which venous inflammation is propagated, is *towards* the heart; but, according to Dr. R. Lee, the extension in phlegmasia dolens is *always* retrograde.

Cases of puerperal phlegmasia dolens are recorded in which we have

positive assertions that no uterine phlebitis existed. In Dr. Davis's case we have the testimony of that distinguished surgeon, Mr. Lawrence, that the uterus was healthy, of the natural size, without vascular congestion, and the vessels everywhere contracted and empty; and we think it very unlikely that so unequivocal a disease as uterine phlebitis, would have been overlooked by him. Again, Dr. Simpson, of Edinburgh, has published the case of a lady who died five weeks after her confinement, of phlegmasia dolens of the left arm and left side of the face. "On opening the body, the vena innominata of the left side, and its affluent trunks, were found entirely obstructed by coagulable lymph." "The uterus was nearly of its natural dimensions, and did not present any traces of disease."* Without quoting more cases, which it would be easy to do, there are sufficient to prove the occurrence of the disease without the possibility of extension from the uterus, inasmuch as it was in a healthy state; and it is probably within the experience of most to have met with cases in which phlegmasia dolens existed only below the knee, the vessels of the thigh being apparently quite free from disease: so that, even if uterine phlebitis existed, we must find some other explanation of its mode of propagation.

3. The continuity of the disease, as we have already said, seems at first sight a proof of extension; but on reflection this is by no means certain. If the reader will turn back to the last of Dr. Mackenzie's experiments, he will find that a continuous disease was produced in the left iliac and femoral veins, although a ligature was interposed, and although the disease in the iliac veins was obviously excited by the entrance of the foreign fluid; and that in the femoral, after that fluid had gone the round of the circulation.

4. The influence of uterine phlebitis upon other organs is at variance with this theory of retrograde extension in puerperal phlegmasia dolens. No one has more ably illustrated the peculiarity of this disease in exciting secondary inflammation of distant organs or tissues, with purulent deposits, &c., than Dr. R. Lee. He also attributes the uterine phlebitis to the absorption of morbid matter, and the secondary disorders are generally supposed to result from this vitiation of the blood, and not from extension. Is it logical, then, to make crural phlebitis consequent on uterine phlebitis an exception to this rule, without more proof than their continuity?

5. Several circumstances seem to point to a morbid influence in this disease, more general than mere local phlebitis. In our experience we have found that the severity of the general symptoms was not necessarily in proportion to the extent of the local affection; nay, in many cases the danger seemed inversely as to the amount of obstruction, and the only satisfactory explanation pointed to the relative impurity of the circulating fluid. Again, phlegmasia dolens occurs sometimes after the lapse of a considerable period after delivery, at a time when acute uterine phlebitis is not common, or when, if it existed, it may be supposed to be in process of cure: but this is scarcely consistent with its active extension, but might more reasonably be attributed to some more general influence exerted by the previous inflammation. Lastly, phlegmasia dolens has

* Edinburgh Monthly Journal, Sept. 1847, p. 912.

occurred in connexion with epidemic diseases whose notable effect is the deterioration of the circulating fluid.

6. Thus we seem driven to the conclusion, that although it is possible that in some cases puerperal phlegmasia dolens may be an extension of uterine phlebitis; yet that in most, if not all cases, there is ground for attributing much influence to the vitiation of the blood, and of some cases it affords the only scientific explanation. It is true that this view is not without its difficulties; it does not explain every peculiarity of the disease: we cannot tell why crural phlebitis is more frequent than the other secondary affections of uterine phlebitis, nor why the lower extremities are more liable to the disease than the upper, nor why a woman who has been the subject of phlegmasia dolens after one labour is more liable to a return after the next, nor why phlebitis from wounds, &c., does not give rise to all the symptoms of phlegmasia dolens, &c.; but neither will Dr. R. Lee's theory explain these cases more satisfactorily.

7. But now let us dismiss the cases of phlegmasia dolens occurring in connexion with delivery, or disease of the uterus, or injury, which are apparently susceptible of explanation either way, and take an example or two where extension was impossible, or in the highest degree improbable. Now, if in such cases there could be no direct influence, it is clear that the disease must either have arisen spontaneously, or indirectly, through the vitiation of the blood; and if from the latter, the conclusion applies with very great force to the explanation of the production of puerperal phlegmasia dolens. Case 54, related by Dr. R. Lee, is entitled "inflammation and obstruction of the lower portion of the vena cava, and the right iliac and femoral veins, produced by an encephaloid tumour at the upper portion of the chest." The tumour occupied the upper portion of the chest, and a little to the right side of the upper bone of the sternum. The lungs and contents of the abdominal cavity were healthy, except the vena cava (inferior, we suppose), the right iliac and femoral veins. In this case, surely, the influence of the tumour in producing the disease must have been indirect, as it was too distant from the affected veins to act mechanically, or by extension, upon them. The inference that it must have acted through the circulation is unavoidable. Again, Dr. Mayne, of Dublin, has recorded some very interesting cases, in which phlegmasia dolens occurred in the course of dysentery, and he has kindly afforded us the opportunity of examining one such case at present under his care. The symptoms were well-marked and characteristic, and after death the condition of the crural veins was precisely the same as we find it in dissections of cases of puerperal phlegmasia dolens; but the most minute and careful examination failed in tracing the inflammation to the veins of the intestines, or to those of the uterus. Here again, although a local disease existed, we are obliged to conclude that it produced its effects indirectly. Lastly, well-marked phlegmasia dolens has occurred in the course of fever, of which Dr, Mackenzie has collected ten cases. Our friend, Surgeon Hughes, of Dublin, mentioned to us that this happened with a patient of his: the symptoms were quite characteristic, and the consequences just as we see them in patients recovering from the disease after delivery. In this case, and we infer also in most of the others, there was no local disease from which the phlegmasia dolens could extend.

We might multiply examples of this kind, but we think these are sufficient for our purpose, which was, to discover what element they had in common with each other, and with the cases of puerperal phlegmasia dolens.

8. Now, in these three latter classes, it is not assuming too much, to suppose that this common element is some vitiation of the blood; there is good reason for believing that this occurs in carcinoma, in certain forms of dysentery, and in fever. Nor can we doubt that a similar effect is produced in certain cases, at least, of puerperal uterine phlebitis: and if we infer this to be so in cases of secondary deposits, there does not appear to be any sound reason for supposing that it may not also be the case in phlebitis complicated with phlegmasia dolens. And, moreover, the supposition of a diseased condition of the blood will meet the difficulties we have mentioned as involved in the theory of propagation by extension; whether the co-existing inflammation of the uterine and crural veins be continuous or not, and it will explain why the general affection may be inversely as the local disease. So that our conclusion would be, that inflammation and obstruction of the veins is an essential morbid condition of phlegmasia dolens; that in puerperal phlegmasia dolens, the uterine and crural veins are both frequently, though not always, affected; but that it is not probable that the disease is generally propagated by extension; and lastly, that in the majority of cases, if not in all, it is highly probable that the primary morbid element is a vitiated condition of the blood.

Fleetwood Churchill.

Review VI.

1. *Die Asthmatischen Krankheiten der Kinder.* Eine Monographie vom Dr. Wilhelm Johann Theodor Mauch, Physikus in der Stadt und dem Amte Rendsburg. Erster Theil. *Vom Verhältnisse der Thymus beim Asthma.—Berlin,* 1853. 8vo, pp. 181.

The Asthmatic Affections of Children. A Monograph by Dr. W. J. T. Mauch, Official Medical Officer in the Town and District of Rendsburg. Part I. *On the Relation of the Thymus to Asthma.—Berlin,* 1853. 8vo, pp. 181.

2. *Journal für Kinderkrankheiten.* Herausgegeben von Drn. Behrend and Hildebrand. Band xix., Heft 1 und 2.—*Erlangen,* 1852. 8vo, pp. 156.

Journal for the Diseases of Children. Edited by Drs. Behrend and Hildebrand. Vol. xix., Numbers 1 and 2.—*Erlangen,* 1852. 8vo, pp. 156.

It may not be superfluous to recall to the recollection of our readers that the term *asthma*, as applied by many to certain disturbances of the respiratory function in children, bears somewhat a different import to when used in reference to adults. It is true, that some of the "asthmatic affections" of children are considered by many to be purely functional neuroses, or dependent upon changes in the nervous system of a mere dynamic character, and in this way, it may be said, that a coalition

is formed with the true or spasmodic asthma of after years. But even with this admission, it must be granted that, bad as the term is, with respect to children, when least vaguely employed, it indicates a disorder whose symptoms, progress, and result are very different from those of spasmodic asthma. We say, when least vaguely employed, for we must allow that by some the term is applied so generally and unadvisedly as to embrace any form of dyspnœa, and upon whatsoever it may be dependent. We are not even satisfied with how far Dr. Mauch is to be exempted from having rather a lax view on the point in question. He nowhere gives a general definition of infantile asthma or asthmatic diseases, though we are willing, from the incompleteness of his treatise, to give him the benefit of our doubt, and assume he will fairly demarcate his "asthmatic affections" from the more frequent hindrances to normal respiration, and which are of a secondary, and often subsidiary, character. A common and more definite interpretation of the title of our author's work is, to include under it those forms of dyspnœa and abnormal respiration which are attended with, or even caused by, more or less closure (from spasm) of the glottis, stridulous breathing, or "crowing noise," and sometimes accompanied or followed by partial, or even general, convulsions. Their leading features are phreno-glottism, or laryngismus cum stridore, carpopadal contractions, and eclampsia. It is not absolutely necessary, however, that the closure of the glottis be accompanied by the "crowing noise," or breathing *cum stridore*, for in both slight and fatal cases these may be wanting; nor, although carpopadal contractions exist in a majority of cases, and general convulsions follow in some, is it essential that such automatic movements coincide with the laryngismus Hence it will be seen, that under the "asthmatic diseases" of children are embraced "Millar's," "Kopp's," and "Hirsch's asthma," "thymic asthma," "laryngismus stridulus," "false croup," "spasm of the glottis," "angina stridula," "inward fits" of some, "cerebral croup" of others, and even the "suffocative catarrh" of a few writers. While some of the above appellations, and others we might have named, must be regarded as synonymous, or as applied to the same affection, others by no means hold the same relation, as very different diseases are implied, or at least ought to be. Upon this point, however, much confusion has existed, as Dr. Copland has well shown,* some writers having considered one and the same affection to be really involved in the various accounts given by different writers under the above names. Although, as Porter and Marshall Hall have remarked, laryngismus occurs in the adult, yet in general its causes and complications are such as to give us no trouble in separating it from the phreno-glottism *cum stridore* of children. The nearest approach, and, indeed, a very near one, recorded as occurring in the adult, to one of the "asthmatic affections" of children included in the before-mentioned terms, is shown in Dr. Budd's three cases, and in another alluded to by Dr. John Reid, in his able little work on Infantile Laryngismus. We may also mention that Dr. Joy, in his article on Spasm of the Glottis,† indicates a simulation in the adult of another of these infantile diseases. On the other hand, the question may be asked, does a disease analo-

* Art., Larynx and Trachea: Dictionary of Practical Medicine, vol. ii. p. 675, et seq.
† Cyclopædia of Practical Medicine, vol. ii. p. 351.

gous to the true spasmodic asthma of grown persons ever occur in children? We are not aware* of any trustworthy observations upon this point in particular, and we conceive it would be quite a mistake to consider any of the different writers *de asthmate infantum* as capable of assisting us to a solution of the question. Dr. Forbes,† it is true, remarks, that asthma "occurs in every stage of life, but most commonly in middle life, being rare in infancy and childhood," and which would imply that it does occur in children. Heberden‡ has not neglected to inform us also that "the first fit of the asthma has been experienced at all times from the earliest infancy."

Could we be sure here that all fallacy was out of the question (from some of the "asthmatic affections" we are considering), we should feel the more inclined to urge upon attention the fact, that we have met with three cases, the patients' ages ranging from 5 to 10 years, for which we could find no other name than spasmodic asthma, the general symptoms were those of the affection in adults, we could discover no sufficient proofs of pressure from enlarged thoracic glands, of bronchitis, tracheitis, pertussis, &c., and they were alleviated by the use of valerian, æther, hyoscyamus, and ipecacuanha.

It is the intention of Dr. Mauch to enter first into a minute investigation of the morbid anatomic appearances which have been found in children who have suffered from "asthmatic diseases," and then, upon such a pathologic basis, to discuss the various forms of infantile asthma, and finally to consider their treatment We certainly cannot say that our author has (to use a common expression) "rushed into print." He tells us he was originally led to pay especial attention to the maladies in question in consequence of "an epidemic of Kopp's asthma" occurring at Schleswig in 1837-38, and where he then resided. That a few years after he intended to have written an essay in competition for a prize at Hamburg, but circumstances interfering, he has now re-arranged his previously collected materials, extended his views, and offers the present and first part of his treatise to the profession. Dr. Mauch also informs us, that, unless circumstances over which he has no control, should interfere, another part shall appear every successive year for five years, when the work will be completed. Had the author to undertake another 'Copland's Dictionary,' we might have much sympathy with this dignified and cautious style of production; as it is, we can only hope that Dr. Mauch may be left to finish his labours, and we to review them. Whatever we may think of his research, learning, and professional acuteness—and of them all we think highly—if we may be allowed to be judges of German style and composition, we most assuredly cannot say much for him as regards the latter. The analysis of his "first part" has been to us one of the driest and most tiresome literary duties we have for some time performed; his tautology is most wearisome and perplexing, and he must excuse us if we take occasionally some liberty with his text when putting it into an English dress for our readers.

The inaugural dissertation§ of Dr. James Simpson has been allowed

* We would not be too positive, for although we cannot definitely refer to any writer, yet we have an indistinct notion of having met with some remarks in reference to this matter.

† Cyclopædia of Practical Medicine, vol. i. p. 185. ‡ Commentaries, chap. 11.

§ De Asthmate Infantum Spasmodico. Edinburgh, 1761.

the priority of having first directed attention to our present class of diseases; and to the treatise of Dr. Millar* has been awarded the praise of much increasing that attention, and of exciting active controversy here, on the Continent, and in America.† We learn from Dr. Mauch, however (p. 108), that Felix Plater,‡ in 1614, related, under the title, 'Suffocatio a Struma Interna Abscondita circa Jugulum,' the case of a little boy, who died from hindrance to the respiration, which was *cum stridore*, and who evinced no other symptom of disease. The father of the patient having previously lost two other children under similar circumstances, allowed the body of this one to be examined. The thymus gland was found enlarged, spongy, and full of blood, and was assumed, from the pressure it must have exerted, to have been the cause of death. Plater therefore appears not only to have been acquainted with laryngismus stridulus, but to have forestalled Hood and Kopp in assigning the thymic enlargement as its cause. Further, it can be shown that in 1726, an infantile "asthmatic disease" was well known to Verdries, Waldschmitt, and Hert, and was described by the former.§ Great hindrance to the respiration existed, the breathing being sometimes stridulous or whistling, at other times noisy or rushing, and with a difficult gasp or whoop. These and other laryngeal symptoms were superadded to decided inflammatory action going on in the lower air-passages, and the affection which was prevalent near Giessen, seems most nearly to have approached to "Millar's asthma." The thymus, however, is stated to have been found strikingly enlarged, "so that it invaded the whole anterior region of the chest, and was filled either with a tough mucus or thick blood." From the time of Millar to 1827, several writers described different forms of "asthmatic disease," accompanied by croupal, stridulous, or laryngic breathing or crowing, often associated with partial or general convulsions, and sometimes causing sudden and unexpected death. In the year just mentioned, Mr. Hood, of Kilmarnock, drew specific attention to enlargement of the thymus as productive of one form of disorder; and in 1829, Kopp, of Hanau, read a paper before an association at Heidelberg, on what he denominated *thymic asthma*. In later years, very various opinions have been held as to the part which the thymus gland plays in the production of any of the so-called asthmas, laryngismus, and sudden death from closure of the glottis. Some are strong believers in the existence of a thymic asthma; others deny the occurrence of such an affection; whilst a third party admit of its occasional occurrence, but maintain "still all cases of laryngeal suffocation appearing spontaneously in children do not proceed from enlargement either of this gland or of any other.|| One or two high authorities have even asserted that the enlargement of the thymus, when found, is rather an effect, than the cause, of the disease it has been brought forward to explain. Notwithstanding what has been said by some late writers, we are of opinion that an enlarged thymus *may* give rise to an affection fairly coming under our present class of diseases,

* Observations on the Asthma and on the Hooping Cough, with an Appendix, &c. London, 1769.

† See Reid, op. cit.

‡ Observat. in hom. affect. &c. Basiliæ.

§ Diss. Inaug. de asthmate puerorum quod vulgo vocant "Das Rücheln der Kleinen Kinder." Præside J. M. Verdries. Auct. J. C. Waldschmitt. Giessæ, 1726.

|| Copland, op. cit.

nay, has been known to do so. But to seek in this organ a *general* cause for phreno-glottism, laryngismus, &c. &c. is, to our mind, quite as wrong as to deny the former proposition; and we think that Dr. Reid* has scarcely done justice by the case he quotes from Dr. West, in not adding to his quotations from the latter, Dr. West's further remark, "notwithstanding some points of difference between this case and those in which spasm of the glottis had been induced by a different cause, yet we recognise in it the grand symptoms of the affection."† The present and first part of Dr. Mauch's work is devoted to the consideration "of the thymus gland in relation to asthma." He has made its intention so elastic, however, that the work before us might, with equal justice, be called one upon the general pathology of the organ both in the adult and child. Whilst we find an excess of labour on the one hand, we meet with a deficiency of explicitness on the other; for we have been puzzled to make out what amount of weight Dr. Mauch really does allow to morbid conditions of the thymus in the production of "asthma." We have *cases* by wholesale, sometimes brought forward apparently to prove an affirmative; at others, to show, as it appears to us, a negative. We have even imagined that the author regarded the enlargement of the thymus as an effect rather than a cause of the "asthma." We give Dr. Mauch full credit for his research and his collection of instances, commented upon as they are often very acutely, but we really do wish he had given us such a *résumé* as is the admirable practice with so many of the French writers. It may be that such will be found in the last part, which we have already stated is —if nothing prevents—to make its appearance five years hence.

As the author does not enter upon the minute or comparative anatomy of the thymus, we need not either, the more particularly as Mr. Simon's admirable essay, or the critical review of it in one of our predecessors,‡ is most probably in the hands of our readers. We shall commence with the second chapter, which treats "of the function of the thymus."

"When the position and structure of the thymus, and particularly the anatomic disposition of its bloodvessels, are fully considered, it will appear that its most probable destination is to serve in the fœtus as a *diverticulum sanguinis* for the lungs during their minority. . . . .

"This view of the matter receives further support from the circumstance that, with the performance of the above duty, another is seen going hand-in-hand with it, and which the thymus performs, not only in the fœtus, but even after birth. This is seen to consist in its filling up in the fœtus the otherwise empty space which would be left in the cavity of the chest, from the lungs not being as yet expanded by air and blood. Such a space, however, the lungs will require, and all at once, as soon after the beginning of respiration, they become so suddenly and greatly expanded and increased, that a much larger space is requisite for their augmented volume, than was the case before birth. Even after the latter, the thymus performs a like office, though in a somewhat modified way, and continues to do so until a proper relation is gradually established between the capacity of the thorax and the extension of the organs the latter contains. . . . . It is in the highest degree probable, that in cases where a determinate want of relation between the cavity of the thorax and its contained organs exists, the thymus, not only partially, but even sometimes in the totality of its extension, may continue to endure. Not only may it persist to the years of puberty, but, if

* Op. cit., p. 60. † Lectures, p. 269.
‡ British and Foreign Medical Review, vol. xx. p. 159.

the existing mis-relation continues, even to adult or most advanced age; and though it may have attained a considerable size, may not give rise to the slightest inconvenience." (pp. 16, 19.)

Those acquainted with the essay of Mr. Simon, will not fail to remark how very differently are the uses and function of the thymus therein represented. We pass to the third chapter, on "The Diseases of the Thymus." Before the time of Lieutaud (1767), observations and cases in illustration of the diseases of the organ were scattered singly through numerous volumes. This writer collected about twenty cases, the histories of which, however, were so unsatisfactorily detailed, that, as Dr. Mauch (p. 29) remarks, the labour of Lieutaud was soon quite forgotten, even by pathologists of good repute. To Dr. Baillie, and to his translator into German, Th. Sœmmering, is the credit due of having revived attention to the diseases of the thymus.

"No organ of our body has excited so much attention in late years as the thymus, which has found in Haugsted, Astley Cooper, and John Simon, three monographists, whose concurrent observations have contributed very much to a more perfect acquaintance with its protean nature, particularly in an anatomic and pathologic view." (p. 40.)

"Purulent destruction of the gland" is first alluded to by the author (p. 30). This has been described by Pauli, Lieutaud, and Sauvages. By the former two, a syphilitic origin was ascribed to it.

"Such destruction of the thymus is by no means, however, produced alone by the venereal dyscrasia, but may arise as a consequence of other diseases connected with a dissolved condition of the fluids of the body." (p. 32.)

Dr. Mauch observes, in a note, that—

"The very different characters under which syphilis has appeared amongst us at different periods, render it very probable that the 'forms of destruction' also which it gives rise to, or tends towards, must and can alter very much in the course of time." (p. 32.)

To the observations of Dr. Mauch we would add mention of those of M. Paul Dubois, who has found in several children, born of syphilitic parents, and themselves presenting evidences of constitutional contamination, a particular condition of the thymus—viz., the presence of small disseminated nodules of inflammation and suppuration. M. Dubois, along with M. Bouchut, is inclined to regard it as due to the operation of the syphilitic poison. M. Depaul has followed in the steps of M. Dubois, coinciding with the opinion of the latter in regard to the small collections of pus in the thymus, and affirms, also, that the "lobular nodosities," observed by Billard, Olivier, Husson, and Cruveilhier, in the lung of new-born children often of syphilitic parents, and assumed to be the result of inflammatory action taking place before birth, are, in reality, the "plastic nodules" of a special lesion *never* observed unconnected with signs of syphilis in the parents or the children. They are, therefore, to be regarded as a specific alteration belonging to constitutional syphilis. A distinguished obstetrician, M. Cazeau, has in particular opposed these views; but as we should be swerving from our main object to enter into more detail, we must content ourselves with referring the reader to the seventeenth volume of the 'Journal fur Kinderkraukheiten,' where they will find

a report of the discussion, &c. of the Academie Médicale in Paris on the subject, as also some observations of Dr. Behrend on the matter.

"Scrofulous degeneration in adults" is next alluded to. This morbid condition has been frequently mentioned by writers, but, in the opinion of the author, has been regarded more as a pathologic curiosity than as anything else. Several cases are noticed by Dr. Mauch, which prove the detrimental influence upon the respiratory and circulatory apparatuses produced by the pressure of the scrofulously-enlarged gland. The function of respiration is first hindered; then, as the enlargement increases, the heart and large vessels feel its influence, and œdema or dropsy follow. Sometimes the nerves become pressed upon, or involved by adhesions with the growth of the gland; in the latter case, according to the observations of Ferrers, if the position of the body is suddenly changed, so as to strain the nerve, as it were, "spasms or cramp" follow. Loss of voice, and ulceration of the contiguous parts—as of the œsophagus—from the pressure, may likewise ensue. From the analysis of Dr. Mauch's cases, it appears that a strong tendency to somnolency has been a marked attendant, upon several instances, of the affection in adults. Allan Burns, more than thirty years ago, hazarded the opinion, that the enlarged and indurated thymus, in addition to the evils it may exert upon the functions already noticed, might produce direct and severe disturbance of the assimilative and nutritive ones. He assumed that it might so press upon the left subclavian vein, as to hinder the entrance of the chyle into it from the thoracic duct. The particular state of the mesenteric glands, and other circumstances often found in connexion with the disease of the thymus, and which were supposed by Burns to bear out his opinion, are explained, and we think satisfactorily, by Dr. Mauch in a different way. The northern surgeon also suggested that, in certain cases, the enlarged gland might be removed by operation; whilst Putegnat thinks he has seen it, when projecting above the sternum, diminish and disappear from the local application of iodine. The following and tenth section is concerned with the scrofulous affection, as it occurs from the second to the twentieth year of life:

"I have already, in § 8, pointed out how easily scrofulous deposits may be formed in the thymus, especially during the first years of life; and I shall now place together a considerable number of cases in which, not only in children but in young persons of both sexes, up to the twentieth year of age, the gland has been observed scrofulously hardened. In almost all these cases, disturbed respiration, most variable in form, has been present." (p. 52.)

After the detail of numerous cases, the author observes how they prove that—

"The older practitioners, especially since Richa's time, adopted the opinion of there existing a close connexion between the asthmatic attacks of young children (*pueris* in opposition *infantibus*) and the coincidently enlarged thymus. The enlargement of the organ must then, however, have coincided with a morbid alteration of its tissue, and the so-altered thymus, by its pressure upon neighbouring parts, disturbed the respiration, &c. The observations of late years have proved this theory to be perfectly correct generally; but they have also shown, that it is not alone the pressure upon the air-passages which operates detrimentally on the economy whether of children or of adults, and that the thymus must attain a *certain* size, hardness, and extension, in order to produce, by its mechanical operation upon contiguous parts, lœthal, or even detrimental influences." (p. 73.)

"Since the time, however, that Kopp broached the doctrine that *asthma thymicum* has for its immediate cause too great extension of the thymus numerous disciples have come forward in favour of the new doctrine, but with too much enthusiasm, and maintaining, without the least reservation, that every hypertrophy of the organ must always, and under every circumstance, have as its consequence visible hinderance to the respiration. They have denied, without further ado, the rectitude of observations made by several practitioners, in which it is affirmed that, though the thymus was found hardened and enlarged, the children had suffered no disturbance of respiration." (p. 77.)

In connexion with "pyogenesis in the thymus of children under two years of age," Dr. Mauch rejects the cases of Sauvages, Naumann, Vogel, Billard, and Veron, as illustrative of the results of acute inflammation of the organ. He affirms that he knows of no satisfactorily recorded case of *inflammatio suppurativa* of the gland, or *thymelcosis.* Haugsted, we may remark, quotes a case of inflammatory softening from Portal, and another from Mason, where the thymic abscess is stated to have opened into the trachea; whilst Hasse observes, that "inflammation of this gland is, in point of fact, very problematical."* Alluding to Veron's cases, in which pus was found in the serous cavities, as also in the thymus, Dr. Mauch observes, that these purulent collections cannot be regarded in the light of abscesses, the consequences of suppurative inflammation, "seeing that they were neither enclosed nor secreted by a special (secerning) membrane." (p. 79.)

"It is, therefore, difficult to conceive that they were originally formed in the thymus, where Veron found them; it is with much greater probability to be considered, that the pus originated in another part of the system, became absorbed, and was again deposited at or carried by the circulation to those localities where it was afterwards met with. But it is not even necessary to assume that the actual inflammation and secretion of pus took place in the system of the child or fœtus, since they might just as well have ensued in that of the mother, and the purulent matter have passed from the latter into the former by means of the circulation, as is not so very rarely the case as respects the aëriform or gaseous collections in new-born children."† (p. 80.)

This explanation of the matter may be satisfactory to Dr. Mauch, but we confess that it is not so to us; and we think that, without going so far as to maintain the occurrence of simple acute inflammation in the thymus of the maturer fœtus, the views advocated by MM. Dubois, Depaul, and Bouchut (and to which we have before alluded), in all probability, indicate the solution of the difficulty.

A single case (quoted from Portal) is recorded, of a collection of serum in the thymus of a boy two years old. The next subject coming before us is, "induration" of the gland before the second year after birth.

"Before the time of the Scot, Alexander Wood, not a single observation exists (so far as I am aware) relative to the thymus having been found enlarged and indurated in the bodies of children dying whilst at the breast. To Wood, without question, belongs the credit of having directed attention to the fact, that such enlargement and induration, when sufficient to exert marked pressure upon the veins conveying the blood from the head, may, under such circumstances, sometimes give rise to a hydrocephalic condition, and, at the same time, not rarely

* Sydenham Society's edition.

† Das Emphysem in den Lungen neugeborner Kinder. Von W. J. T. Mauch. Hamburg, 1841.

be accompanied by asthmatic trouble. In England, scrofulous induration of all forms, and so of the thymus, must be far more frequent in children than in other European countries, since *scrophulosis* is there a far wider spread evil." (p. 81.)

In connexion with the existence of an enlarged &c. thymus, and effusion into the ventricles, Dr. Mauch has collected quite a museum of examples. But the mere fact of the two co-existing, does not establish a chain of cause and effect. To our minds, the cases are very rare and exceptional in which either chronic or acute (as it is called) hydrocephalus owe their origin to the pressure of a tumour on the vessels of the head or neck. The one we believe due generally to a chronic inflammation of the lining membrane of the ventricles,* or, before birth, to an abnormal activity of the meningeal vessels, causing a superabundant quantity of the cerebro-rachidian fluid to be effused:† the other to the influence of the scrofulous diathesis, and which sufficiently explains why it should not be unlikely that the particular condition of the thymus alluded to may now and then be found in connexion with one of the lesions of granular meningitis.

Though the occurrence of simple, acute, and suppurative inflammation of the thymus is doubted by our author, he believes in that of such a "*dispositio inflammatoria*," accompanied by an effusion of lymph, as to entitle him to treat of "adhesive inflammation" of the gland. This gives rise to pure hypertrophy of it, a form of thymic disease neither in its origin, nor in itself, hitherto properly described by pathologists. According to Dr. Mauch (p. 107), it was confounded by Kopp himself with simple distension of the thymus from sanguineous stasis.

"When the deposited lymph is freely traversed by blood-vessels, the texture of the thymus has a carnified character, and is far less compressible than in its natural condition." (p. 121.)

"The carnified, firm, scarcely now compressible substance of the gland, not only contributes in maintaining and increasing the asthmatic troubles already existing, but in many cases may even originate new forms of the latter." (p. 123.)

Dr. Mauch is of opinion, we may fairly conclude from analogy, that by means of absorption an hypertrophied thymus may return to its normal condition again.

"Distension of the Thymus by Blood" in young children, was first noticed by Verdriess, of Giessen, in the beginning of the last century.

"Since the time that Kopp and many other pathologists have regarded enlargement of the thymus as the special cause of *asthma thymicum*, marked attention has undoubtedly been paid to the condition of the gland in children dying, or supposed to have died, from the disease in question. But in the majority of cases, observers have contented themselves with merely weighing the organ, or taking its measurements. Rarely, and indeed only when very evident degeneration of it was present, was its internal pathologic condition particularly noticed. Of its frequent distension by blood, scarcely any account was taken, so that the entire theory of Verdriess, in connexion with this point, fell tantamountly into entire oblivion. Since the time of Kopp, also, very many pathologists regarded this condition of the thymus as identical with *asthma thymicum*. But since that, in late times, several large hospitals for children have been erected, and medical men

* See the review of Weber, &c., in vol. x. p. 15, et seq.
† Also of Churchill, &c., vol. vi. p. 140, et seq.

have bestowed far greater attention to the study of pœdiatrics: it has resulted that unprejudiced and attentive investigation has proved, without gainsay, that not only in children dying from *asthma Koppii* has the thymus been found very greatly distended, and sometimes also morbidly altered, but that a like condition of the thoracic gland is not rarely seen in the bodies of other children, suffering quite other forms of asthma, at the same age: and that the stasis of the blood in the system of the *vena cava superior*, necessarily present in all asthmatic affections, must be regarded as the peculiar and immediate cause of this enlargement and distension of the thymus." (p. 131.)

We shall pass over the sections treating of Valentin's, Meckel's, &c., observations, and of the enlargement of the thymus in connexion with atelectasis. The nineteenth section is entitled "Pathologic Alterations of the Thymus, as the result of different Asthmatic Affections of Young Children." The effects of the paroxysms of pertussis, bronchi-pneumonia, and simple acute pneumonia, illustrate the section, concluded by the author in the following words:

"In the meantime, there is the less necessity for my adducing observations of this kind, as I intend in one of the following parts of this treatise (when the proximate relations and treatment of asthma will be considered), to bring forward a host of facts, which will sufficiently support this view [viz., so far as we can make out, that the enlargement or distension, &c., of the thymus, is caused by the sanguineous congestion or stasis induced by asthmatic paroxysms of the above diseases]. Utterly erroneous, therefore, is the view taken by those who regard the enlargement of the thymus as the cause of pure asthma Koppii; and I shall endeavour in the next chapter to point out how it has been that medical men, and particularly those of Germany, have gradually come to erect such a theory, opposed, as it is, to the principles of sound physiology and pathology, and in part to maintain it firmly even at the present day." (p. 146.)

It is from the occurrence of such passages as the above, in connexion with others, such as our quotation from p. 73 of Dr. Mauch's treatise, that we have felt puzzled in arriving at the author's real opinion as to the influence and frequency of diseased conditions of the thymus in giving rise to disturbances of the respiration. The next chapter commences with the consideration of Meckel's views on the thymus acting as a vicarious organ for the lungs in certain diseases; and in particular, the connexion of an enlarged gland with malformations of the heart. The effect of persistency of the opening of Botal, and its result, cyanosis, in producing stasis in, and consequent distension of, the thymus, followed by "thymic asthma," are regarded by the author as of rather exceptional occurrence. The opinions of Schallgruber, a former medical jurist of Grätz, next pass under Dr. Mauch's criticism. According to the former, it is very probable that in many cases of sudden death in infants, where the child has been supposed to have been suffocated accidentally during the night by the nurse or mother, or where violence has been thought to have been had recourse to, and yet, on inspection of the body, no sufficient evidence of such violence can be found, but the thymus is enlarged, the morbid condition of the latter, and its effects, have been the cause of death. In many of the cases recorded, the size of the gland was such that Dr. Mauch thinks it very difficult to conceive how it could have exerted such lœthal pressure: and since, in others more carefully investigated, it has been shown particularly by Berg, that an atelectasic condition of the

lungs is in general present, the fatal event should rather be associated with such a condition than with the state of the thymus. We may remark that Schallgruber himself was perfectly well aware of the complication of atelectasis with the above cases, since he says that "the lungs, without exception, in such children remain undeveloped," but he regarded the "excessive growth" of the thymus as the hinderance to the expansion of the pulmonary organs, as well as the immediate cause of death. The doctrine taught by Schallgruber has been supported by Krombholtz, but it is evident that the latter was led to false conclusions, from reasoning as follows: Having examined ten new-born children, some of whom were prematurely born, and found the weight of the thymus to be respectively 23, 31, 78, 97, 106, 135, 159, 185, 208, and 255 grains; he formed a low standard of the normal or medium weight from this, for in a case of death in which the gland weighed 208 grains, he thought an abnormal size and weight were indicated, and ascribed the fatal event to the influence of the supposed enlarged thymus. And so in cases of "suffocation," where he found the weight of the gland going beyond his standard, he regarded the growth of it to have been excessive, and ascribed the death to it accordingly; whereas, in reality, no abnormality existed as regards this organ in any of his cases.

"The thymus in the human fœtus during the first months of pregnancy occupies a very small amount of space, but in the ninth month its growth is so excessively rapid, that a very great difference in the weight and size of it will be found in children who have completed the full period of gestation and those born prematurely. But at the birth of the former the weight of the thymus will be found to vary considerably; the more trustworthy observers of our time, however, allot to it 240 grains as a normal weight, though there exists ample proof of the organ in such children having been found on the one hand much heavier, and on the other very much lighter than this." (p. 1.)

The twenty-third and concluding section of the present part of Dr. Mauch's treatise is occupied with "The Views of Kopp and his School."

"Kopp, following Haller, took the normal weight of the thymus to be from 160 to 180 grains in a ripe newly-born child, and maintained that when this limit was to any extent surpassed, the gland might operate so detrimentally upon the heart and lungs of young children, as to hinder the free circulation of the blood. Such would more readily occur also in the fœtal state, since the heart is then placed more towards the right side and where the thymus is situated than afterwards. After birth the gland is liable (according to Kopp), from various circumstances, such as crying, swallowing, &c., to become from time to time strongly compressed, and appressed upon the heart and large vessels; by this the minor circulation is disturbed, and the normal rhythm of the respiration interrupted. During such interruption and presence of less amount of air in the morbidly affected larynx, a shrill cry with an acute tone is formed, and which is to be regarded as the characteristic symptom during life of *asthma thymicum*. A paroxysm such as this only continues until the continuous power of the heart and the lungs, and of the vessels, again overcomes the present hinderance, and the thymus is consequently compressed into a smaller amount of room. But if it should happen that the influence of the thymus is not thus early and sufficiently overcome, and as a result, the arrestation of the minor circulation be too far prolonged, suffocation or apoplexy will necessarily ensue." (p. 170.)

After discussing various modifications of Kopp's theory, advanced by

several writers, and the opposing statements of other pathologists, Dr. Mauch closes his treatise in the following words:

"Under such circumstances, we really cannot find fault with the Frenchman Brunache for regarding the theory of the German physicians, upon the mode broached by Kopp of explaining the origin of thymic asthma, as a species of mystification. Nevertheless, although in Germany at the present time many thoughtful practitioners have become satisfied that 'Kopp's asthma' is a disease dependent upon too great nervous irritability, yet the *practicus currens* still seeks its origin, for the most part, in a hypertrophied condition of the thymus gland." (p. 181.)

As we have yet but only one part out of six, of which the work is to be composed when finished, we can form no accurate judgment as to what will be the real value of Dr. Mauch's labours; but we can recommend the present portion of them to those desirous of knowing what has been recorded by different pathologists concerning the pathology of the thymus both in children and in adults.

The first paper in the present number of the 'Journal für Kinderkrankheiten' is one by Dr. Lederer, on "Spasmus Glottidis," and which he calls the "Gordian-knot of pædiatrics." As the author is assistant-physician at the Hospital for Children at Vienna, and 4400 patientshave passed under his notice during a single year, his views are entitled, of course, to a hearing. Two forms of the "spasm" may be distinguished, viz., a tonic and a clonic form, and these may pass into each other. A comparison of the cases of "asthma" occurring during January, 1850, to the end of June, 1852, affords the following results: Out of the 96 cases, cranio-tabes existed in 92, in part associated with hypertrophy of the brain, with acute hydrocephalus in one instance, and with chronic œdema of the brain in another. In 3 cases, rachitis of the trunk and of the extremities, without cranio-tabes, existed; and in one case was present congenital malformation of the heart. In 8 children who died from "asthma" in the hospital, and were examined after death, the thymus was found to be very small in 2, of a normal size in 4, and apparently enlarged in 2; whilst in a patient exhibiting marked hypertrophy of the gland not a symptom of "asthma" was evinced during life. Considering, then, that neither the thymus, bronchial glands, condition of the larynx, trachea, epiglottis, and brain were, in the author's opinion, able to be assigned as the cause of the affection, and since, in 92 out of 96 cases, cranio-tabes coincided with the "asthma," the former must be regarded as the most frequent predisposing cause of the latter.

Dr. Borchman, of Landshut, contributes a long article on "croup," the chief points inculcated in which are, 1st. That there is only one affection rightly so called. 2nd. That this is a diphtheritic inflammation. 3rd. That it is quite immaterial whether the latter be of a "primary" or "secondary," "ascending" or "descending," character, and that its peculiar danger lies in the production of spasm of the muscles of the glottis. 4th. That the local application of ice to the neck is one of the most intense antiphlogistic measures we can employ in the disorder. An interesting case is recorded as having occurred to Dr. Riecke, and in which a little girl of 4 years of age suddenly died on the night-stool after having complained of abdominal pain, colic, diarrhœa, &c. On examination of the body, 7 balls, formed of intertwisted lumbrici, were found in the jejunum,

each ball containing from 8 to 13 worms, the total number being 88. Two worms were also found in the stomach. The mucous membrane appeared reddened at the spots where the "worm balls" were situated.

Some interesting remarks on "Sausage poisoning" will likewise be found in the present journal, as also the able paper of M. Rilliet on "Invagination," and some clinical observations of Professor Guillot on "Nurses and Sucklings."

W. Hughes Willshire.

Review VII.

1. *Lehrbuch der Nervenkrankheiten des Menschen.* Von Moritz Heinrich Romberg, M.D.—*Berlin*, 1851.

A Manual of the Nervous Diseases of Man. By Moritz Heinrich Romberg, M.D. Translated and Edited by Edward H. Sieveking, M.D., for the Sydenham Society.

2. *Clinical Lectures on Paralysis, Disease of the Brain, and other Affections of the Nervous System.* By Robert Bentley Todd, M.D., F.R.S.—*London*, 1854.

*Romberg's treatise is but a part of an intended whole, and includes only the derangements of sensibility and motility. It is his purpose, at a future time, to complete the subject by adding the logo-neuroses (Λογος, mens. ratio), diseases of the mental activity, and the tropho-neuroses (τροφη, nutritio), diseases of nutrition; inflammation, degeneration, morbid growths, &c.; or, as the translator in his preface terms them, the diseases of the formative foci. Although the work is incomplete, its reputation in Germany is such, that it is now passing through a third edition. The members of the Sydenham Society in its perusal will find, like travellers in a distant country, that although the geography is new, the principal features in the landscape are the same as they have at home so long admired, Romberg having transplanted largely from our literature into his pages.

As Romberg adopts a physiological order in the classification of his subjects, he first treats of the derangements of those nerves which convey inwards impressions to the centres in which they are implanted, whether sympathetic ganglia, spinal cord, or encephalon. These constitute the *hyperæsthesiæ* and the *anæsthesiæ* Abnormal sensibility is not, however, a character common to the whole class, but we may infer that morbid conditions similar to those which, in sensitive nerves, are appreciated by alteration of sensibility, occur in other incident nerves, although differently manifested, as, for instance, by reflex action on the muscular tissue.

In his recapitulation of the laws which govern sensation, Romberg dilates principally upon that of eccentric phenomena; or, as we would rather call it, of peripher*ad* interpretation. This law of our constitution is now so well known as to require no particular illustration. Both physiologists and metaphysicians have discoursed largely upon it,

* The writer must apologize for the delay in this notice of the translation of Romberg's work. Circumstances over which he had no control prevented its appearance at the time intended, now nearly a year ago.

seeing it forms the foundation of our knowledge of external things, and is, as Reid called it in his treatise, "the principle of common sense." Well known, however, as the law is, it will probably ever remain an inexplicable problem why our consciousness should refer outwards to the external world, as in the instance of the nerves of special sense, the olfactory, optic, and acoustic; or to their extremities, as in the instance of the nerves of common sensation,—changes occurring in them at any part of their course, or in the centres into which they are inserted.

There is, however, another law to consider with this; for although it explains a large proportion of the instances of apparent centrifugal excitation of sensitive nerves, yet we recognise this latter condition as actually occurring, and from recent advances in nervous physiology have reason to conclude, that both in sensitive and motor nerves irritation at any part of their course induces a polar state, which is transmitted in both directions along the trunk;—the terms centripetal and centrifugal not being exclusively applicable to the one or the other set of fibres, so far as the changes in the nervous tissue are concerned. Romberg has not, in this part of his work, recognised, with sufficient distinctness, such a law, although nervous pathology affords many apparent illustrations of it. Hunter, even, in his day, distinguished these reflected impressions from the phenomena of peripherad interpretation, fancifully referring the latter to a "delusive principle in the animal economy," leading us astray from the real subject or seat of the disease, whilst the former are by him correctly referred to *true sympathy*:—"I am confident," says he, "that I can fix my attention on any part until I have a sensation in it."

Clinical experience, almost daily, affords instances of not only increased sensibility from a centrifugal excitation of sensitive nerves, but of alterations in the circulation and nutrition of the part following upon it. Such phenomena are the very *ignes fatui* of diagnosis, and occasionally mislead the most careful practitioners.

Since Romberg includes, in his 'Neuroses of Sensibility,' the abnormal conditions of every class of incident nerves, his hyperæsthesiæ are much more comprehensive than writers generally make them, as is shown by the following classification:

"*First Order.*—Hyperæsthesiæ from irritation of the *Nerves*:—

1. Cutaneous hyperæsthesiæ: neuralgia, pruritus, ardor.
2. Muscular hyperæsthesiæ: neuralgia muscularis, vertigo.
3. Hyperæsthesiæ of the vagus: gastrodynia neuralgica, bulimia, polydipsia.
4. Hyperæsthesiæ of the special senses: hyperæsthesia optica, acoustica, olfactoria, gustatoria.

"*Second Order.*—Hyperæsthesiæ from irritation of the *Central Organs*:—

1. Hyperæsthesiæ of the sympathetic ganglia:
   - α. Hyperæsthesiæ of the cardiac plexus.
   - β. " " solar plexus.
   - γ. " " mesenteric plexus.
   - δ. " " hypogastric plexus.
   - ε. " " spermatic plexus.
2. Hyperæsthesiæ of the spinal cord: spinal neuralgia.
3. Hyperæsthesiæ of the brain: cerebral neuralgia.
   Psychical hyperæsthesiæ.

"The character which these affections have in common is exalted irritability and increased irritation of the sensitive or centripetal nerves. The expression of this irritation is either psychical, one of consciousness, a sensation,—or motor, a reflex movement,—or both at the same time. The sensation varies according to the peculiar sphere of the affected nerve. Hyperæsthesia of the cutaneous nerves is manifested by pain in its various modifications; that of the nerves of special sense by phantasms. The part taken by the brain, as the psychical organ in sensation, is not only receptive but also reactive.* If the imagination dwells upon the sensation, the latter becomes more intense and more defined; the power of imagination may create sensations, as proved by the feeling of nausea or prurigo, and a morbid condition termed hypochondriasis."

The whole of the preliminary remarks on this part of the subject, though containing nothing particularly new, are yet of great value as pointing out, that a neuralgic condition, or that which is equivalent to it, may express itself in other ways than by pain—many forms of muscular spasm, or other kinds of abnormal muscular irritability being due to hyperæsthesia of some one or more incident nerves, which may yet in themselves manifest nothing abnormal, and thus our attention being drawn to the more prominent symptoms, the actual cause is overlooked. To use the language of Marshall Hall, we must investigate every part of the diastaltic arc, from the nervous expansions in the skin and mucous membranes, through the whole course of their tracts up to the centre, and thence outwards to the muscles, before we can arrive at a correct diagnosis. In the range of the sympathetic, Romberg finds similar phenomena, and attributes many derangements of the cardiac rhythm, intestinal spasm, and ischuria, to hyperæsthesia of the sympathetic tracts, and the ganglia in connexion with them.

Respecting the ætiology of such affections, no addition is here made to our ordinary knowledge, but we would remark, that with so comprehensive a definition as this writer adopts, he is hardly justified in stating that childhood is entirely exempt from these affections, since early age is prone not only to that form which results in spasm or convulsion, but irritable retina and colic are met with in their most marked forms at this period of life.

Cutaneous Hyperæsthesiæ.—The effects which sometimes follow injuries of the sensitive nerves are adduced by Romberg as typical illustrations of the neuralgic condition. We think, however, that most observers will not quite agree with him in this. Such cases are characterized by many peculiarities. The anatomical changes at the seat of injury keep up a special form of excitement in the affected nerve-trunk, which is apt to induce more extensive sympathies than occur in other forms of neuralgia. A trifling wound of a cutaneous nerve which heals readily, may be followed, after a short interval, not only by severe pain in the adjacent branches, but the local excitation may slowly propagate itself until the whole sensitive system becomes involved, and severe constitutional irritation and convulsions and death ensue. The cases quoted by Romberg, and which are most of them taken from English writers, bear out these remarks. Simplicity of cause does not warrant our inferring simplicity in the effects, at least in living tissues; and though a mechanical injury may be more accurately appreciated than one from other causes, yet it is doubtful if its results are always the most simple, and whether we may not

rather regard the cases occurring from anæmia and miasm as more typical, being, to use the words of Romberg, "the prayer of the nerve for healthy blood."

Neuralgia of the Fifth Pair.—Although Romberg objects to an ætiological classification of his subjects, upon the ground that we know so little of causation in neuralgic affections, yet we feel that in no division is an effort towards such classification more necessary than in the affections of the fifth pair. *Per causas scire* is, indeed, here *vere scire*, for so various are the conditions out of which painful affections of the fifth nerve arise, that if we can go no further than the prominent symptom of pain, and its anatomical distribution, our practice must be a chaos of empirical formulas. It is the want of this classification, so far as it could be made, which gives to this chapter an unsatisfactory character, the writer appearing sometimes to limit his remarks to the chronic atypic forms occurring after the middle period of life, and constituting, as we generally understand it at the bedside, the true tic douloureux; and at others, including such as arise from miasmatic, rheumatic, gouty, or sympathetic conditions.

The particulars of a case of facial neuralgia on the left side, of twenty-six years' duration, will be read with interest. As in similar cases, the cranial bones were thickened, the dura mater adherent, and the cerebral convolutions atrophied; there was also sub-arachnoid and ventricular effusion; general arterial disease, with aneurism of the left carotid in the cavernous sinus, compressing the Casserian ganglion and pituitary body; with atrophy of the left fifth nerve. The following are the criteria given for the diagnosis of facial neuralgia of the atypic form:

"1. The relation of pain to time and space; being limited to certain distributions of the nerve, and occurring in paroxysms with few intervals. 2. The peculiarity of the exciting causes of the pain. 3. The sensitiveness of the affected surface to unexpected and slight contact, especially if the disease be of long standing, whilst strong pressure not only does not increase, but often diminishes, the pain. 4. The preference shown by it for mature age—rarely occurring before thirty years. 5. The rarity of the disease, which must make us hesitate in the diagnosis; for whilst painful sympathetic sensations in the face belong to the daily routine of practice, such cases, except those of the acute and typical kind, are rare even in great cities."

Pruritus, formicatio.—Although, as Romberg remarks, hyperæsthesia of the cutaneous nerves is not characterized by pain only, but also occasionally by other manifestations of sensation, as itching, creeping, and the like, yet it is rare for these affections in an idiopathic form to be so intense, or to last for so long a time, as to give them, by themselves, a pathological status. It is notorious that, for the most part, they are but symptomatic of morbid conditions of the blood, or of obvious disorders of the nutrition of the skin, which, from its structure and physiological relations, is predisposed to such irritation. Notwithstanding the authority of Romberg, we think it will be admitted that most of the forms of prurigo, however admissible into a scientific category of nervous derangements, yet practically, are better treated of with the diseases of which they are symptomatic. Therapeutics contradict the opinion of their being in themselves substantial conditions, at least when they are so formidable as to call for treatment; the principal excep-

tions being certain cases of reflected irritation upon the pudendal nerves from sexual excitement. We can well understand, why Romberg should say, that the treatment of pruritus is very unsatisfactory, and so it must continue to be if we are content to place so high a pathological value upon a symptom, to the exclusion of a closer appreciation of the causes which give rise to it.

Hyperæsthesiæ of the Nerves of Muscular Sense.—As the muscles receive sensitive nerves, we are not only in health made aware of the changes they undergo, but in disease their pathological conditions are represented by the same means, and hence the feeling of stiffness, or of lassitude and the pains of cramps and lumbago. Romberg, however, attributes more than such symptoms to disorder of the muscular sense. After recapitulating the advances in modern physiology in respect to the kind of knowledge obtained through the action of muscles, as of space, direction, force, and weight, the pathology of vertigo is introduced, which, according to this author, is a hyperæsthesia of the sensitive nerves supplying the muscles. We cannot see the evidence of the truth of the position thus assumed. and are still disposed to attribute such a symptom to derangement of the sensorium commune, and in particular to that part of it connected with the optic centres. The propriety of the term, "nerves of muscular sense," is doubtful, as favouring the assumption of a special energy in them, whereby they give us a sensation, which, like sight or hearing, is simple and uncompounded, though it is more than probable that the so-called muscular sense is a complex subjective state, requiring no inconsiderable analysis for its elucidation. As the subject has no great practical application, we may spare our readers a further discussion of it, nor need we bring before them the different forms of vertigo—titubans, vacillans, caduca, tenebricosa, or gyrosa—or treat of the difference between vertigo and nutation; for though Romberg abrogates transcendentalism, we think he has scarcely avoided it in this part of his subject.

Hyperæsthesia of the Vagus.—The incident nerves of the vagus are subject to hyperæsthetic states, characterized by various phenomena, according to the branches affected, and the character of the exciting cause. Hyperæsthesiæ of the respiratory branches may give rise to cough, or a sense of oppression about the chest, and incubus or nightmare is probably a subjective condition referrible to the same branches. The gastric branches are more prone to hyperæsthesia than the pulmonary. Romberg enumerates three varieties; the first, a sense of constriction in the pharynx constituting the globus hystericus, in which the sensitive rather than the motor fibres are affected, the assumption of spasm in the œsophagus not being supported by observation; the second, pyrosis, is "a sense of heat and soreness passing from the stomach up through the œsophagus, accompanied or not with a flow of gastric juice; and the third form, gastrodynia neuralgica, of which we have the best examples in gouty, hysterical, and hypochondriacal subjects. It is often difficult, if not sometimes impossible, to distinguish neuralgic affections of the stomach from chronic ulceration, especially when this is situated at the lesser curvature; nor can we agree with Romberg, that the diagnosis is easy, even when the ulceration is

extensive. Within the last six months, two cases, in men at the middle period of life, have fallen under our notice, where, as proved by post-mortem examination, very extensive chronic ulceration, extending from the lesser curvature, destroying the whole of the coats of the stomach, and dividing the trunk of the left pneumogastric, had during life been regarded as forms of gastric neuralgia. In one the existence of lead in the system had further complicated the diagnosis. Death in both took place suddenly from rupture and effusion into the peritoneum. It is often remarkable with what indistinct symptoms, and with what little affection of the general health, these chronic ulcerations of the stomach may progress, until a fatal termination by rupture occurs. According to Romberg, the following may be enumerated as *safe* means for establishing a diagnosis: 1. *The effect produced by pressure.* In neuralgic gastrodynia, the most superficial touch instantly produces pain, whilst firm compression produces no inconvenience, but even relief; the reverse is the case in structural lesions. 2. *The function of digestion* is disturbed, and the secretions altered, and painful in disorganization; painless in neuralgia. 3. *The sympathetic affections* are characteristic of hyperæsthesia, but absent in organic disease. The occurrence of thirst as a symptom of irritation of the pneumogastric is a fact of some interest. In a case, lately under our care, of inflammation, and sloughing of the glands and cellular tissue in the superior triangular space of the neck on the right side, following an attack of scarlatina in a child aged 3 years and 9 months, the trunk of the vagus was exposed, trismus came on, soon followed by general tetanic rigidity, opisthotonos, and death. One of the most remarkable symptoms was the urgent thirst complained of, although, from the proximity of the irritation to the salivary glands, the mouth was always moist with secretion. Polydipsia is rarely, however, to be regarded as the result of hyperæsthesia of the vagus, as it probably was in this case, and practically neither this symptom nor bulimia admits of being treated of as an isolated nervous affection. We have not space to enter into the inquiry of the dependence of hunger and thirst upon the vagus, Reid's experiments showing that distension of the stomach, œsophagus, and pharynx, after its division in animals, is not dependent upon a loss of the sense of satiety, but rather upon paralysis, are not referred to by Romberg.

Hyperæsthesiæ of the Nerves of Special Sense.—These are characterized by phenomena varying according to the nerve affected—being, in the case of the optic, luminous spectra; of the acoustic, different kinds of sound; of the olfactory, odours, generally more or less unpleasant; in the case of the gustatory nerve, where patients complain of bitterness, saltness, or acidity in the mouth, it is difficult to separate the effects of disordered secretions from subjective states of the nerve itself. It is difficult to determine what part the nerve takes in the production of these symptoms, and how much depends upon the activity of the hemispheric ganglia, which we recognise as the centres of the intellectual operations. When, therefore, Romberg asserts that insanity may be caused by optical hyperæsthesia, there is much doubt if he does not mistake disorders of the internal sense —"the mind's eye," as Shakspeare calls it—for irritation of the optic nerve. Upon the diagnosis in question, Romberg writes as follows:

"It is important, in reference to a diagnosis between central and peripheral dis-

ease, to ascertain the shape, and especially the sharpness, of the contour. If the retina be the part affected, the images present a sharp outline, which may be so marked, that, as the retina in this condition is sensible of itself, the patient may perceive parts of it or its bloodvessels, or even the movement of the blood-corpuscles. The patients themselves see the phenomenon with such precision, that they attempt to give a delineation of it. Ruete, who has laboured successfully to establish the physiological relations of ophthalmic disease, has described such luminous phenomena as originating in the circumference of the optic nerve, and invariably commencing externally to the optic axis, in the direction corresponding to the optic nerve. They generally have a semilunar form; they are less frequently circular, with an asteroid border, and a silvery, golden, or coloured margin. The rays are in a constant state of intense cilia-like movement, appearing most frequently in the right eye, less frequently in the left, and, least of all, in both eyes at once; they follow the movements of the eye, and as the organ adapts itself to nearer or more remote objects, they increase in apparent size and intensity. If the distance of the object remains the same, they retain the same size only for a short time; they then begin to extend and to move to one side, and almost backwards; at last, the fire seems to dart from the forehead, the temple, and the zygoma, if the cause of irritation resides in the vicinity of the ciliary portion of the retina, as the lines of vision of this part pass through the localities alluded to. As the lines of vision approach the ciliary portion, the phenomenon ceases, because the nervous fibres terminate here. Its duration varies from several minutes to several hours, and it may return after weeks, months, and even years. The phenomenon remains luminous, whether the eye is open or closed, except that it is rather more vivid and distinct in the latter case. Vision is not impaired; even during the persistence of the phantasmata, objects that are not covered by the rays appear distinct and well marked; but those that interfere with them become indistinct, and look as if surrounded by a halo. Violent headache generally ensues, accompanied by a sense of tension and weight in the eye. If the central organ of the optic nerve is the seat of the affection, the luminous pictures are less defined; they resemble the images of dreams, are removed from the circle of human or animal forms, and, from the field of vision presenting no depth, appear disposed upon a flat surface."

When figures of men or animals, or other complex phantasmata, occur, or where words or sentences, or distinct voices, are heard, there can be no doubt of the implication of the intellectual centres. Delirium tremens affords us frequent illustrations. In this disease, the optical and acoustic phenomena are strikingly distinct, and even during convalescence, continue to trouble the patient. Not long since, we had under our care a man who, after an attack of delirium tremens, was annoyed for nearly five weeks by the constant repetition of the same sentence in his right ear; and many patients have told us how vividly, for days after an attack, they could see the objects which presented themselves during the stage of excitement.

Subjective sensations of the olfactory nerves are among the more rare clinical phenomena. Romberg remarks, that the cases hitherto published are from central disease. Fœtid gases evolved from the respiratory and digestive organs, and delay of the secretions in the sinuses connected with the nasal cavities, may lead to a fallacy in diagnosis.

Hyperæsthesia of the Sympathetic Tracts.—Under this head Romberg arranges angina pectoris, cœliac neuralgia, mesenteric neuralgia, and neuralgia of the hypogastric and spermatic plexuses. Though an unlimited amount of time has been bestowed upon the subject by the best observers, our anatomical and physiological knowledge of the sympathetic is not so far advanced as to afford a sure basis for pathological theories. Romberg

is therefore careful to use the word "tracts;" for though we can advance but little that is certain respecting the sympathetic as an independent system of nerves, we are able to recognise with some distinctness the affections of the cerebro-spinal fibres which are commingled with it. Hyperæsthesia of these tracts may give rise to reflected irritation, both of the voluntary and involuntary muscles, and probably, also, to an alteration in the functions of secretion and excretion. Romberg believes that the peculiar sense of fainting and annihilation accompanying the pain, and which finds an expression in the vascular system, and in the pale and haggard appearance of the patient, in so-called gout in the stomach, is pathognomonic of its seat being in the nerves of the cœliac plexus, as distinguished from the branches of the vagus. He also admits that the development of carcinoma in the stomach may be a direct result of long-continued cœliac neuralgia.

"We do not possess positive proof," he says, "of the transition of hyperæsthesia to organic derangement, still it may be assumed with much probability to take place. Even during the neuralgic paroxysm, certain phenomena in the circulation and secretion are known to occur. Neuralgia of the cœliac ganglia often precedes the development of carcinoma for many years. Frequent repetition and permanence of hyperæsthesiæ influence organic formation, and give rise to structural changes."

Hyperæsthesia of the Spinal Cord.—This title will no doubt lead the reader to suppose that Romberg is a believer in all that has been written in this country on so-called spinal irritation; he is, on the contrary, one of the greatest opponents of this once fashionable nervous theory:

"In the first edition of the present work (1840), I pointed out that, to satisfy the critical demands of science, we required more data about the disease in question, and that medical men, after rashly affirming its existence, had delighted in paradoxical and hypothetical arguments about it. I have, since that time, both in my private and hospital practice, subjected the question of spinal irritation to a rigid inquiry, and have arrived at the conclusion, that beyond the knowledge of some irradiated sensations and reflex phenomena, it has contributed nothing either to physiology or pathology, nor is it likely to do so. The patients generally being females, much deception is practised upon the medical man; and, in addition to this, the whole range of hysterical and neuralgic affections has been made available to obtain the materials for interpreting, or rather for misinterpreting, the affection. In hysteria, the tendency to sympathetic and reflex phenomena is frequently so much exalted, that irritation of the skin of the posterior as well as of the anterior surface of the trunk, or the sternum, the abdomen, and especially pressure applied to the ovarian region, is calculated to produce the entire group of neuralgic and spasmodic phenomena."

The opinion here expressed is in accordance with that of the best observers in this country and in France. Valleix's excellent article on dorso-intercostal neuralgia is worthy of perusal in reference to this subject. According to Romberg, hyperæsthesia of the spinal cord occurs in two forms: either as a spinal malady, manifested by reflex action, without any sympathy of the brain as an organ of perception, as we observe in poisoning by strychnia in tetanus and hysteria,—or as excitement, which becomes the direct object of the consciousness, and is characterized by pain in its different varieties. In our experience, where morbid changes—as softening, and the like—have been limited to the cord itself,

the pain complained of has not been great, and has occurred only in the direction of those nerve-trunks which have had their origin at or above, but contiguous to, the seat of injury.

We have watched cases of softening of the cord, where the protective coverings have been healthy, without observing any modifications of sensibility referrible to the seat of disease. It is, for the most part, only where the membranes, bones, or ligaments, and the posterior roots of the nerves are implicated, that there is an expression of pain in any degree of severity. The following case, as bearing upon spinal irritation, appears to us worthy of recording:

In February, 1853, we were requested to see a gentleman, æt. 48, who was said to be labouring under a very painful rheumatic affection, which he dated from cold and fatigue at the funeral of the Duke of Wellington. He had been treated in various ways without benefit, and for three weeks previous to our visit had taken lemon-juice in large quantities, and was the worse for it. The following report was made of his condition:—He is about the middle height; moderately well nourished; light complexion; pale; of a nervous and rather desponding temperament; skin cool; extremities cold; no swelling in any of the joints; tongue moist, with white fur; bowels regular; urine high coloured, normal in quantity, and without deposit. Complains of great tenderness at a point on the left heel, over the pisiform bone of the right wrist, and at a circumscribed spot over the acromion process of either side. Percussion of the spines of the vertebræ in the dorsal region gave him great pain, and on pressing, even lightly, the seventh, he almost fainted. There was no pain or sense of tightness in the course of the intercostal nerves; no weakness of the legs or of the sphincters. At a subsequent visit, when, on taking his hand, the spot over the pisiform bone was accidentally touched, he gave a sudden shout of pain, quite startling. On examining the part, though there was no appreciable affection of the tissue, yet he asserted that the pain on touching it was indescribable. The neuralgic sensibility in the course of the spine, and especially at the seventh dorsal vertebra, continued unabated. Counter-irritation by blisters, the vapour douche, sedatives, diaphoretics, and tonics, produced no impression on the hyperæsthesia. The pain was far more intense when the skin over the seventh dorsal vertebra was manipulated than is excited when there is disease of the parts beneath. As the spring advanced, with change of air, and zinc in gradually increasing doses, and a liberal allowance of wine, he slowly recovered, though there was a partial recurrence of the symptoms for six or seven months.

Hyperæsthesia of the Brain. Encephalalgia.—(Hirnschmertz, brain-pain.)—The translator has headed the chapter "Cephalalgia," which term is too comprehensive, including all the painful affections to which the head is subject, whilst Romberg here treats only of those which are dependent upon lesions of the encephalon itself. Although, as is seen from accidents in men and experiments on animals, the brain substance may be extensively injured without giving rise to pain, yet it is notorious, in clinical experience, that no symptom is more frequent when the same parts become the seat of disease. Romberg rejects the theory of its arising from insensible organs becoming sensible during a state of irritation and inflammation:

"In what manner," he says, "are we to reconcile these facts with the results of physiological experiments? Assuredly not by the theory that insensible organs become sensitive during a state of irritation and inflammation: we can have no stronger proof of the fallaciousness of this argument than is presented by the fact, of a portion of brain which has been forced out of the skull, and is attacked with inflammation, continuing painless. We obtain more information from the effect produced by pressure exerted upon the brain. Both hemispheres may be extirpated in a living animal without inducing paralysis, whilst the injection of a few drachms of fluid into the cranial cavity produces hemiplegia of the opposite side. This is owing to the uniform pressure exerted upon the distant motor nerves. In the same way, irritants act upon the sensitive points, though the seat of the former may be at a great distance; the different parts of the brain seem, in a measure, to be responsible for one another, and probably the organs of the living brain—an organ, be it remembered, enclosed in unyielding walls—may contribute to the propagation of an irritant influence: surgical observations of penetrating wounds of the skull, as well as the effect already alluded to, of holding the breath, appear to confirm this."

Admitting the truth of these remarks, we cannot believe that pressure and sympathy will explain all the cases of encephalalgia depending upon lesions of the brain-tissue. As Romberg himself states, hæmorrhage into the brain is less frequently accompanied by pain than other diseases of the organ, though this cannot occur without producing increased pressure, and more rapidly than arises from development of tumours or abscess. On the other hand, white softening, in which there is an atrophy of the part, and a condition the converse of pressure, is often attended with great pain. We do not, therefore, reject the theory alluded to as summarily as Romberg does.

In general, neither the seat or character of the disease can be determined by the seat or character of the pain, if we except the instances in which the membranes become involved by extension of the disease:

"The question as to whether the seat of the pain corresponds to the seat of the disease may be generally answered in the negative. Circumscribed alterations not unfrequently give rise to pain in the entire head, or in one half; derangement in the cerebellum is often characterized by pain in the forehead; in some patients the pain shifts about, while others, again, always feel the pain at that part of the head which happens to occupy the lowest position. The sensation as if the head were ready to burst accompanies diseases of trifling extent; while tumours of great bulk excite no feelings at all commensurate with their size. The situation of the disease affords no satisfactory key to the intensity and varieties of the pain."

Although Romberg considers pain as a symptom taken by itself to be but of little value in the diagnosis of cerebral lesions, yet, together with the history of the case, it is a most important phenomenon. By considering the combination, the relation, and succession of the other symptoms, the occurrence of derangement in the intellectual or voluntary centres, the increase of the pain after sleep, the character of the intermissions, in which, if there be organic disease, the patient is rarely free from all traces as well as the exciting cause, the age of the patient, and, we may add, the co-existence of other diseases—rigidity of the arteries, degeneration of the kidneys, &c., we may generally complete the diagnosis.

There is a form of headache occurring with disease of the arteries, to which Romberg does not especially allude. It has been noticed by Dr.

Bright, and in some particulars corresponds with the cervico-occipital neuralgia of Valleix, the pain being principally referred to the occiput and upwards in the course of the occipital nerves. A few months since, a case of this kind fell under our care, in a gentleman aged 56, who for *four years* had been troubled, almost daily, with pain in the head, of a dull heavy character, but sometimes more acute, and referred to the region of the head above described. The heart-sounds were natural, the digestive, secretive, and excretive organs healthy: he was of the middle stature, moderately nourished, and apparently of a nervous temperament, in part no doubt owing to his continued suffering. Most physicians of eminence in London had tried their therapeutics for the relief of his symptoms, but in vain, and had given various accounts of their cause. One day, in the midst of his usual avocations, the pain became more severe, he quickly became insensible, and died in five hours. The arteries of the brain were extensively ossified, and death had occurred from hæmorrhage, with laceration into the lateral ventricles. Dr. Bright supposed that in these cases the pain might be due to irritation of the occipital nerves by ossific deposit in the vertebral arteries, with which they are in contact.

Neuralgia Cerebralis.—Under this term Romberg treats of hemicrania. We have great doubt if hemicrania depend primarily upon a neuralgic condition of the brain. From observation of its phenomena in others, and frequent experience of it ourselves, we have been led to regard it as originating in the sympathetic system of nerves, whereby the tonicity of the arterial trunks, distributed to the brain, is lessened, and for the time an atonic condition of the cerebral circulation produced. The experiments of Reid, Bernard, and others, upon the cervical ganglia of the sympathetic, seem to be in favour of such an hypothesis. The more prominent and characteristic symptoms of an attack, the imperfection of vision, the throbbing of the carotids, the sense of weight, tension, and heat about the head, the congestion of the eye on the affected side, the nausea, vomiting, and coldness of the extremities: as well as the phenomena of its subsidence, and the entire freedom of the interval, accord with such a theory. On the other hand, the intellectal faculties are unaffected, except so far as their exercise is attended by an increased severity of the symptoms.

Hyperæsthesia Psychica.

"I apply the term hyperæsthesia psychica," says Romberg, "to that frame of mind in which abnormal sensations are excited and maintained by directing the attention to impressions; it is commonly called hypochondriasis."

The classification of hypochondriasis with mental disorders has been admitted by the best authorities. There is often, however, in these cases, great difficulty in estimating the *rapport du moral et du physique.* Romberg treats of the subject but from one point of view, and sees in the functional and organic changes which often occur with hypochondriasis, only so many results of a psychical hyperæsthesia.

"Structural changes supervene in those organs which have hitherto been the stage upon which so many sensations, determined or increased by the patient's attention, have appeared. Thus a new period of the disease commences. The digestive organs, the liver, the stomach, the spleen, and intestinal canal, most frequently become the seat of the affection. Tumefaction or induration of the

organs may be recognised by palpitation and percussion. Hypertrophy and dilatation attack the heart, and tubercle is deposited in the lungs. The organs of sense and the brain are less frequently affected. The constitutional symptoms correspond to the local malady. The complexion alters, nutrition becomes impaired, and hectic follows; but through the chaos of symptoms, the red line of hypochondriasis may be traced; the sensations that are really present are increased, and new ones are generated by the power of the imagination. The older physicians distinguished between hypochondria cum materiâ and hypochondria sine materiâ, a distinction which is still admitted. The former is considered to depend upon somatic changes, and especially upon disturbances in the organs of digestion and the abdominal circulation; while the latter is stated to be an independent mental affection. No great acumen is required to see the mistake here committed, it results from the history of the disease having been severed, and the successive stages having been treated as distinct conditions. Hypochondriasis can only be said to exist, if the mind creates new sensations, which, in their turn, give rise to nutritive derangements."

We have not space to enter upon a discussion of these views; yet, with all deference to so high an authority, we believe that the influence of functional and organic derangements in producing hypochondriasis is often more than a mere assumption, as Romberg regards it; and in a large proportion of cases, the success of treatment depends upon our viewing the disease under the two aspects of the physical and the moral.

With this rapid survey of the *Hyperæsthesiæ*, we must conclude our remarks on the neuroses of sensibility, and would occupy the remainder of the space allotted to us in reviewing the *Acineses* or paralytic affections.

Dr. Todd justly remarks, that medical men are apt to speak of palsy as if it constituted the whole essence of these maladies, though in no instance has morbid anatomy done us better service than in demonstrating how various is the nature and seat of the lesions which are attended with this symptom. Taken by itself, it is notoriously a phenomenon of most uncertain import, and whose value can alone be determined by preceding and concomitant circumstances. It is from the investigation of these that our knowledge of paralytic diseases and their treatment has so steadily advanced during the last twenty years. Modern pathology, aided by the light of physiological laws, to the establishment of which she, in return, has contributed so much, has extended the scope of the inquiry from the local lesion to the more general causes which give rise to it, whether in the circulatory, the nutritive, or the secretive systems. Since the discovery of Bright, showing the dependence of many cerebral disorders upon renal derangement, constituting a large proportion of the cases of serous and simple apoplexy of former writers, we have made a great advance in our knowledge of the other forms, by observing the conditions of the arterial system. The immediate and constant dependence of the nervous tissue for its activity upon a constant arterial supply, is a point of equal physiological and pathological interest. The brain is dependent upon the blood, not only for its supplies of nutrient material, but its irritability must be maintained by a succession of oxygenated corpuscles, brought by the minuteness of the channels into the closest contact with its tissue. If this fail, we have loss of function, transient, or more enduring, according to the persistence of the vascular disturbance.

This is true in a like manner, though in a less degree, with the muscular tissue. Romberg gives a remarkable instance of arteritis, attended with paraplegic symptoms, to illustrate such a form of muscular paralysis. The case is, however, too complicated to admit of any rigid analysis; but the pathological principle insisted upon is universally admitted. During the course of last year, we were consulted in the case of a lady, aged thirty-two, who, subsequent to an injury of the middle finger of the left hand, began to suffer from paralytic weakness and wasting of the muscles of the left arm. The wound, though slight, had suppurated, and the whole finger was for a short time inflamed; but there were no symptoms of extension of the disease up the arm. On examination, the brachial artery was found hard and tender, and obstructed up to the point at which the sub-scapular branches are given off. The temperature of the left hand was 77° Fahr., whilst that of the right was 97½°. The left forearm was one inch less than the right. At the end of nine months, the pulse could be again distinguished at the wrist, and the muscles had nearly regained their usual power and size.

The conservatism of this important function of vascular supply is, for the most part, so admirably provided for, that we are apt to overlook the perfection of the arrangements which subserve to it. The rete mirabile of the pia mater, and the anastomosis of the large vessels at the base of the brain, provide against all general disturbances in this organ; but the supply of the principal ganglionic masses being by branches which have less communication, disease in them is soon followed by defect in the function and nutrition of the parts to which they are distributed. It is not, however, to the arterial branches only that we look for the causes of paralysis from deficient supply; the part played by the heart in these effects is beginning to be appreciated with greater precision; its fibres, its valves, and its orifices have in turn their share in the phenomena, according as the one or the other is affected by disease. Most observers will admit, that, from cerebral anæmia, attended with syncope to syncopal coma, followed by transient paralysis and actual disorganization from deficient supply, the gradations are easy, and, as Stokes says, "if not stages of the same process, at least proceed from one cause."

Dr. Todd, in the lectures before us, points out the intimate connexion of atrophic softening with sanguineous effusion.

"It (white-softening) very frequently,—I incline, indeed," he says, "to believe almost always,—is the precursor of apoplexy, and, therefore, we frequently find in these patches of white-softening one or more clots of blood of various sizes. The artery or arteries leading to the part are diseased; that portion of the brain fails in its nutrition; it passes into the state of white-softening; and the minute vessels, losing the support which they must receive from the firm brain texture, and being themselves often more or less diseased, give way, and allow the blood to escape into the tissue of the brain."

They who have had much clinical experience have had frequent opportunity of noticing the concurrence of these two conditions in different parts of the same brain, not only isolated, but frequently both conjoined. The appreciation of this relation is of great importance in a practical point of view, it having been too readily admitted that effusion of blood into the brain is a sign of vascular determination, rather than,

as it frequently is, of capillary defect. Rigid arteries, granular kidneys, and hypertrophied left ventricle, are not only coincident, but *correlated* conditions, which may induce apoplexy and paralysis, with a very variable character of the local lesion;—a sudden attack of renal coma;—or the equally sudden supervention of paralysis from softening;—or a stroke of the *apoplexie foudroyante.* Had this relation been more commonly recognised, we might have heard less of the uncertainty of diagnosis in cerebral affections, and less expression of disappointment at *post-mortem* examinations, where sometimes much has been expected and but little found.

Dr. Todd's lectures, in many parts, set forth the relations here named with great clearness. Our space does not permit us to enter upon all the points of this wide subject, including capillary degeneration, plugging of arterial trunks by loosened vegetations, spontaneous coagulation in certain dyscrasiæ, arteritis, and endocarditis; on which, through the labours of Paget, Kirkes, Virchow, Todd, Romberg, Ruhle, Simpson, and Stokes, every day is adding to our knowledge.

Reflex Paralysis.—Under this term Romberg comprises such cases of paralysis as arise apparently from a morbid impression made upon the spinal centres through the incident nerves, and especially through such of them as are distributed in the course of the sympathetic tracts to the abdominal viscera. The subject is but little more elucidated by him than it has been by Stanley, Graves, and Stokes. It is one where illustrations are apt to be very fallacious, and where a more minute anatomical inquiry is necessary, in order satisfactorily to establish the negative facts upon which the theory rests. Whilst we write this we would not be understood to be sceptical on the subject; but no one can have perused the many cases which the above authors have brought in support of their views, without-feeling that a large proportion will not stand the test of a rigid inquiry. Without a microscopical examination, the statement that no morbid appearances were discoverable in the cord cannot be accepted as positive evidence that none existed; and, besides, we find, in several of Mr. Stanley's cases which are referred to by Romberg and Graves, indications of the existence of such. In one it is stated, there was "an unusual vascularity of the membranes below the first lumbar vertebra." In another, "some turgescence of the vessels both in the membranes and substance of the lumbar portion, and a few drachms of transparent fluid in the theca, but neither the turgescence of the vessels, nor the effusion of fluid, were sufficient," says the writer, "to explain the paraplegia by pressure on the cord." And in another, "the pia mater covering the lumbar portion of the cord was very vascular, and there was effusion into the theca." For establishing a pathological principle, there is obviously need of more than usual reserve, especially if, as in the present instance, by its ready adoption we incur the risk of regarding as a functional change that which depends upon inflammation, or other nutritive changes.

Whilst we are ready to admit that, through shock, cold, or exhaustion, the motor centres of the cord may become paralyzed, we must express our opinion that reflex paralysis, from enteritis or diseases of the genito-urinary organs, does not rest upon altogether unequivocal evidence. When para-

lysis supervenes upon colic, independently of lead poisoning, it appears to be too hasty a deduction that the former is the cause of the latter. We should rather regard them as sequences, the colic being but the first appreciable phenomenon of the nervous disturbance, which is more probable, since diseases of the cord are often preceded by such symptoms. Comhaire's experiments, alluded to by Romberg, in which extirpation of the kidney in dogs induced a paralytic weakness in the hind leg of the same side, are not of much value, for we daily see atrophy, fungoid disease, and disorganization of these organs, without any such symptoms.

In the last six years, during which our attention has been particularly directed to this subject, two instances of paraplegia following upon stricture of the urethra and disease of the kidneys have come under our observation, where the cord presented nothing abnormal to the unassisted eye beyond slight congestion of the superficial veins; and the surrounding parts, on the usual examination, appeared to be healthy; yet the cord itself was the seat of inflammatory exudation, and in one of the cases, a more careful examination of the spinal veins demonstrated the existence of phlebitis. Romberg makes three sections of this part of his subject: 1. Reflex paralysis, arising from intestinal affections, as colic, enteritis, and dysentery; 2. Reflex paralysis, dependent upon affections of the urinary organs; 3. Paralysis, depending upon affections of the sexual organs. The facts contained in Dr. Churchill's recent article on paralysis occurring during gestation and child-bed, seems to us to render still further doubtful the ready explanation which offers itself in the assumption of reflex paralysis, although he probably draws a different conclusion. Of 22 cases of paralysis occurring during pregnancy, "12 were examples of hemiplegia, 1 of paraplegia, 4 of facial paralysis, 2 of amaurosis, and 3 of deafness." Of 12 cases occurring during or after labour, "5 were cases of complete hemiplegia; in 1, the arm only was affected; 1 was a case of complete paraplegia; in 1 the right, and in 1 the left leg, only was paralyzed; 2 were examples of amaurosis, and 1 of facial paralysis."

It is seen from this that, if we abstract the cases which may probably have arisen from local injury of the nerves during labour, or from conditions of the pelvic organs arising subsequently, paralysis from the spinal centres is not more frequent under these circumstances than other forms which can scarcely be referred to the operation of the law now under consideration.

From the beginning of pregnancy to the end of lactation, we have a fertile soil for the development of nervous affections, both functional and organic; and as the latter, favoured by changes in the blood and circulating system, are apt to come on insidiously, and without any apparent exciting cause, the most rigid criticism is necessary before we can venture to assign them their place, and determine their reflex origin.

Romberg refers hysterical paralysis to this class. According to him, hysteria is a reflex neurosis, proceeding from the genital system being sometimes characterized by an excess of motility and sensibility, and at others, by reflex inactivity, under the various forms of paralysis, either as paraplegia, hemiplegia, or paralysis of single parts.

All will admit that this is an important aspect of hysteria, but so multiplex a subject cannot be comprehended from one point of sight. The

complication of hysterical conditions with moral perversion is more than an accident, as Romberg himself admits; and a large proportion of cases are obviously due, as Brodie has explained, to defect of the will in its objective relations. The curious phenomena referrible to the centres of volition, which may be induced by emotional conditions, even in healthy persons of either sex, have only recently begun to be studied with advantage, and are still too much deformed by the meretricious dress in which mesmerists and others have arrayed them, for the sober eye of philosophy to deign them a look of approval. Dr. Todd, without entering into the pathology of hysterical paralysis, writes as follows:

"I believe," he says, "hysterical paralysis is caused by a depraved nutrition of the nerves of the limb affected, or of some part of the centre of volition. Moral causes, no doubt, exercise an important influence in the production of this state, and the power of the will becomes impaired; but that a depraved state of general nutrition, which tells chiefly upon the nervous system or upon parts of it, is at the foundation of the malady, I think no one can doubt who considers fairly its natural history."

Notwithstanding so high an authority, we doubt if this theory will be so readily admitted. The very occurrence of such lesions would go far to remove the cases into another category; for if they essentially depended upon disordered nutrition, they could hardly admit of being called hysterical. Their sudden accession under mental emotion in persons previously enjoying good health, and their equally sudden disappearance, even though the affected muscles may have wasted by long disease, seem to contradict this theory; and such objections against it are strengthened by that well known fact in their history, that those parts of the body are most prone to hysteric paralysis whose movements are most distinctively volitional, as the arm, leg, organs of voice, and the bladder being often singled out in a very characteristic way, without any reference to anatomical arrangement, and thus contrasting remarkably with paralysis having an organic origin. We suppose, therefore, that Dr. Todd refers to the predisposing influence of general derangements of nutrition, such as we see in anæmia, amenorrhœa, &c., and not to sensible local changes in the nerves or nervous centres.

We regret that the limitation of our space prevents us from following Romberg, section by section, through this part of his subject. He has given a physiological account of the different forms of paralysis with a masterly hand. They who object that there is a deficiency in the practical character of his remarks, and a want in the therapeutical indications, must remember that these are to be supplied in some future volume, when he proposes to enter upon the disorders of nutrition (*bildungskrankheiten*).

We must now take our leave of this treatise. The translator has very ably executed his task, but the work shows many traces of haste in its passage through the press; thus we have "pulse fat" for "pulse flat," "articular" for "auricular," "axiliary" for "axial," "with any" for "without any," "hyposion" for "hypopion," "umblyopic" for "amblyopic," "interior" for "inferior," "deduced in size" for "reduced in size," "distend" for "destined," "having all" for "lining all," and many such like. Perhaps some readers would have been benefited by the transla-

tion of the word "decubitus" into "bed-sore." We would not, however, be hypercritical, and would congratulate our friend, Dr. Sieveking, on the completion of his labour, by which he has placed both the Sydenham Society and the profession under great obligations to him.

Dr. Todd's lectures have, for the most part, already appeared in the 'Medical Times and Gazette.' They are commentaries on cases which have presented themselves in his clinical course. The principal subjects discussed are the diagnosis, pathology, and treatment of hemiplegia, paralysis from lead, facial paralysis, cerebral symptoms from renal disease, syphilitic disease of the dura mater and periosteum, tetanus and trismus, chorea and hysteria. The remarks on hemiplegia are copious, and include the most important points of the subject, as hemiplegia from diseased vessels leading to white-softening and effusion of blood; hemiplegia following epilepsy; choreic hemiplegia; spinal hemiplegia; peripheral hemiplegia, and hysteric hemiplegia. The state of the muscles is particularly alluded to as affording indications for diagnosis and treatment. Three conditions are noted—1. Hemiplegia with relaxed muscles. 2. Hemiplegia with coincident or early supervening rigidity. 3. Hemiplegia with relaxed muscles, followed at a later period by contraction and rigidity. There is also a fourth class, in which the muscles are almost in their normal state. In hemiplegia following epilepsy;—in some cases of hysteric hemiplegia;—in disruption of the fibres from white-softening;—including a large proportion of ordinary apoplectic cases (provided they be not attended with extensive effusion of blood lacerating the brain or extending to the surface), the muscles are relaxed. When the cause of the hemiplegia affects the membranes and surface, and especially if it be of an inflammatory kind, rigidity is present from the beginning, and is indicative of irritation. In many cases of this class, the paralysis is not complete, and the rigidity is increased by an effort of the will. Where effusion of blood lacerates healthy brain texture, there is often with general relaxation of the muscles a slight rigidity of some of the flexors, or this may be excited by extending them roughly.

Cases of primary relaxation, followed at a later period by slow contraction and rigidity, especially of the flexors, are, according to Dr. Todd, due to a process of cicatrization, which he explains as follows:

"At the seat of the original lesion, whether it be simply a white softening or an apoplectic clot, or a red softening, with more or less destruction of the brain substance, there takes place an attempt at cicatrization, more or less perfect. Attendant on this there is a gradual shrinking or contraction of the cerebral matter, which, acting on the neighbouring healthy tissue, keeps up a slow and lingering irritation, which is propagated to the muscles, and excites in them a corresponding gradual contraction; while, at the same time, their nutrition becomes seriously impaired by the want of proper exercise, and the general depressing influence of the lesion."

It will be seen, from the above account of Dr. Todd's views, if they be correctly stated, that when rigidity comes on early it is due to a cause of active irritation. Although this may be generally so, we have in our note-books cases in which rigidity of a most marked and general kind was present from an early period of the attack, and where the lesion was yet of a passive kind, depending upon an obstructed vessel. Where

the softening extends to the surface of the brain, or where it implicates the superficial fibres of the pons Varolii, we have noted an earlier supervention of rigidity than could be attributed to cicatrisation; and in many of the cases where rigidity has come on later, and been most intense, there has been no evidence (post-mortem) of histological changes in the part allied to the production of cicatrix tissue.

A remarkable case of spinal hemiplegia is recorded, depending upon a fibro-cartilaginous growth from the odontoid process, compressing and flattening the cord to the left of the median fissure. At the commencement the paralysis was limited to the left side, affecting the arm more than the leg, and the power of motion more than sensation. The patient was a girl sixteen years of age. The catamenia had been arrested for six or eight months.

The illustrations of hemiplegic paralysis following, and sometimes even preceding, an epileptic attack, are of very great interest. The oversight of such an occurrence has, in our experience, been the source of many fallacies both in diagnosis and prognosis, as well as of serious errors in treatment; effusions of blood or other lesions having been diagnosticated where none existed, and a permanent paralysis prognosticated where the effects have been transient. Dr. Todd thus explains these results:

> "The phenomena of an epileptic fit depend upon a disturbed state of the nervous force in certain parts of the brain—a morbidly excited polarity. . . . . This undue exaltation of the polar force induces, subsequently, a state of depression or exhaustion, not only in the parts primarily affected, but in parts of the brain connected with them, according to the degree of the primitive disturbance. This state of exhaustion is very apt to continue as one of weakened nutrition, in which the brain tissue is more or less in the condition of white softening. If the parts involved in this be the convolutions, mental power, memory, perception, suffer; if deeper parts, as the deeper parts of the white matter of the hemisphere, and the corpora striata and optic thalami, then we have hemiplegic paralysis."

The relation of chorea to paralysis, discussed by Dr. Todd, is an important point in its pathology. It is shown, not only by the occurrence of actual paralysis, but we have observed, in many cases of chorea, wasting of the muscles of the affected side during the continuance of the choreic movements. The treatment of the disease points to the same atonic and exhausted conditions of the nervous centres.

Dr. Todd seems to have had some doubt of the correctness of the term *peripheral hemiplegia*, as applied to the cases he has described under this head, and we participate with him in his misgivings. Such a term is properly applicable only to hemiplegia dependent upon causes affecting the nervous trunks and their branches. Graves drew attention to such cases, and Romberg, as we have seen above, includes them in the class of reflex paralyses.

A striking instance of peripheral paralysis occurred to us, and which we have recorded, where a woman, from her situation as a cook, was exposed to draughts of cold air on the left side of the body, and which, as in facial paralysis from the same cause, led to general hemiplegic weakness and atrophy of the muscles. The cases described by Dr. Todd, as well as the one quoted by him from Cheyne, are hemiplegic but at one stage of their course, and then not completely so. As they progress, symptoms of

paraplegia and general paralysis supervene, and sooner or later there is impairment of the mental faculties.

In the Gulstonian Lectures of 1849, our attention was drawn to this subject, and since that time we have examined a case presenting the symptoms met with in this class;—the brain was wasted;—the cerebro-spinal fluid was in large quantity;—there were spots of brown discoloration in the optic thalami, and the lining membrane of the fourth ventricle was thickened and roughened with translucent granulations. Some of the cases begin with paraplegic rather than with hemiplegic symptoms—as numbness and coldness of the feet. Pathology refers them to atrophy and degeneration of the centres, rather than of the nervous trunks or their expansions.

Although given to students Dr. Todd's Lectures, cannot but be very acceptable to the profession at large, as conveying, in a practical manner, the views of their distinguished author on some of the principal points in the pathology of nervous affections.

William W. Gull.

Review VIII.

1. *Observations on the Mortality of the Scottish Widows' Fund and Life Assurance Society, from* 1815 *to* 1845. By James Begbie, M.D., F.R.S.E.—*Edinburgh,* 1847. pp. 20.
2. *An Investigation of the Deaths in the Standard Assurance Company.* By Robert Christison, M.D., V.P.R.S.E., Professor of Materia Medica in the University of Edinburgh, &c. &c.—*Edinburgh,* 1853. (Reprinted from 'Monthly Journal.') pp. 56.
3. *Observations on the Causes of Death among the Assured of the Scottish Widows' Fund and Life Assurance Society, from* 1846 *to* 1852. By James Begbie, M.D., F.R.S.E., Fellow of the Royal College of Physicians, &c. &c.—*Edinburgh,* 1853. (Reprinted from 'Monthly Journal.') pp. 31.

That the statistics of the causes of death deducible from the records of Life Assurance Societies were likely at some time to prove valuable to the profession, must, we have little doubt, have occurred to many. A certain period of existence, however, was required before the details of the mortality of any single office could be rendered valuable from numbers, even though their accuracy made them otherwise available. Regarding value, too, much improvement has resulted since the institution of the first Life Assurance Society, the Equitable, of London, in 1801: for it is since that period that the business of life assurance has attracted so much attention, embraced so many interests, and, by the devotion of many able and qualified persons, has assumed the important position it now occupies. Along with this improvement in the business of life assurance itself, there has been a great advancement in the capabilities of medical science as connected with, and adapted for it. It is within the same period that some of the most useful of the present means of diagnosis have been discovered, and that others have been perfected; and if we

select the single instance of auscultation, it is probably not going too far to say, that without the discovery of Laënnec the insurance of human life could not have occupied the commanding position it now does. If, then, we consider how much medical science has done, and continues to do, in the business of life assurance; how large a share of the responsibility necessarily involved in the selection of lives, falls upon the physician, it does not seem unreasonable to entertain the hope that for a time the tables may be turned, that the creditor may now become debtor, and that the business of life assurance may be caused to yield some fruits to medical science: thus gratefully affording to our profession a share in benefits which the latter has so largely contributed to bestow.

To Dr. Begbie is due the merit of first directing the attention of the profession to the valuable statistics of mortality contained in the records of life assurance offices, and in order to render such statistics practically useful, to the necessity of making the certificates of the causes of death furnished by different medical men, more trustworthy and complete. Prior to the publication of Dr. Begbie's first report, in 1847, a table, showing the diseases—as certified to the court of directors—of which persons assured by the Equitable Society of London had died during thirty-two years, from 1801 to 1832 inclusive, was published by the learned actuary of that office. The results of this otherwise interesting report cannot, however, be regarded as of any practical value, owing to the many and serious defects in the nosological arrangement adopted by the Equitable, so that, in the words of Mr. Neison, in his very instructive pamphlet on the 'Mortality of the Provident Classes in this Country, and on the Continent,' the paper of Dr. Begbie, to the title of which we have given the first place at the head of this article, was, up to 1853, "the only published document of any importance which gives the mortality from different causes amongst assured lives." The example given in Dr. Begbie's paper has, during the past year, been followed; and we have now to congratulate the profession on the possession of two additional most instructive and valuable documents on this subject; the one, an extended investigation into the mortality of the Scottish Widows' Fund, by the same author; the other, into that of the Standard, by Dr. Christison.

To these two reports, viewed chiefly in their relation to medical statistics, we now propose to direct the attention of our readers. It is in this respect that the nature of the reports mainly differs; that of Dr. Christison being chiefly occupied with the mortality from different diseases as affecting the business of life assurance offices; that of Dr. Begbie, while also directed to this important feature, entering more fully and strictly into medical details, for which greater opportunity was afforded, owing to the larger numbers he had to deal with. In both papers the various causes of death are given with much accuracy; and comments of great practical value, as well to insurance directors as to the physician charged with the selection of lives, are made. We now propose to follow both authors in their remarks upon some of the most frequent causes of mortality.

First of all, let us present a comparative view of the mortality in the two offices, as included in the reports of each.

Scottish Widows' Fund.

Deaths from 1815 to 1846	642
" " 1846 to 1852	690
Total	1332

The deaths during the seven years from 1846 to 1852 inclusive, are thus seen to exceed by 48 the deaths during the previous thirty-one years, or from the period of the Society's first institution. This apparent paradox is, however, easily accounted for, by the rapidly augmenting number of entrants, and the advanced ages of the earlier insurers. It is to the 690 deaths we propose chiefly to refer.

Standard.—The emerged risks referred to by Dr. Christison amount to the much smaller number of 293.

The first great cause of mortality arises from epidemic and infectious diseases. In the Scottish Widows' Fund, 130 of the 690; and in the Standard, 69 of the 293, arose from this source; the chief diseases being continued Fever and Asiatic cholera. From a Fever there occurred, in the experience of the Widows' Fund, 63 deaths, or fully nine per cent. of the gross mortality: from the latter 27; in that of the Standard, from fever 38, and from cholera 20. This is a class of deaths from the scrutiny of which it is not possible to deduce any lessons of instruction whereby to reduce the mortality from it; for it is clear that no foresight or precaution, whether in regard to the family or personal eligibility of the proposer, of universal or even general application, can be adopted so as to guard an insurance office against loss by fever or by cholera. We say of general application, because there are some means of less general or special application which ought to be, and to some extent already have been, adopted for this purpose. For example, there are certain professions and callings, which more than others expose persons to the influence of infectious diseases: and there are certain districts in which infectious diseases, and particularly fevers, are known to be both more prevalent and more frequently fatal than in others. On these points the papers contain much information, and many useful hints.

"It appears necessary, therefore," says Dr. Christison, "for the security of assurance companies, that in the medical and all other professions, necessarily much exposed to infection in great towns, it should be made a condition of acceptance, that the proposer has either already had typhus, or has been freely exposed without taking it." (p. 15.)

In speaking of the mortality from fever in the Widows' Fund, Dr. Begbie remarks:

"Of the 63 deaths, 38 occurred in Scotland, 14 in Ireland, and 11 in England, —a much larger number having taken place in Ireland in proportion to the number of the assured, but not more so than our knowledge of that country had led us to expect." (p. 5.)

At another place the same author observes:

"On the former occasion of our report we were called to remark, that of 54 deaths from fever, one-sixth part occurred among members of the medical profession—an experience which had led the directors to view with suspicion the proposals of medical men who had not previously passed through the disease, or

whose residence or duty subjected them to a more than usual exposure to contagion. It may be owing, in some measure, to the caution thus exercised, that only two deaths are, on this occasion, recorded among the members of the medical profession." (p. 7.)

Of the 63 deaths from fever, no less than 23, or upwards of one-third, Dr. Begbie found to have occurred in 1847, a year when, as is well known, the disease prevailed very generally over the British islands. Taking the total number of deaths from fever in the experience of the Widows' Fund, only 3 occurred between 20 and 30 years of age, 23 between 30 and 40, 38 between 40 and 50, 35 between 50 and 60, 16 between 60 and 70, and 2 after 70 years of age. Of the 38 deaths in the Standard's experience, 3 were between 20 and 30, 8 between 30 and 40, 9 between 40 and 50, 11 between 50 and 60, 4 between 60 and 70, and after that age only 3. Some interesting remarks are made by Dr. Begbie in regard to the period of the disease at which death took place, but on this point we must refer to the paper itself. The experience of both offices in regard to cholera, just as with fever, permits the deduction of little practical instruction, though it affords some interesting points for consideration. Of the 27 deaths in the Widows' Fund, 23 were those of males, and 4 of females. One victim only belonged to the medical profession; of the 27, 10 deaths occurred in Glasgow, a city in which the disease committed great ravages; and not one occurred in Edinburgh, which in 1848 and 1849, though not preserved from cholera, was more leniently dealt with. The opinion that cholera chiefly attacks unsound or enfeebled constitutions, receives no countenance from the experience of the Standard, according to Dr. Christison; but the argument he adduces against it, from the fact of 17 of the 20 victims of the disease being regarded, at the period of their selection for assurance, "above the average, and many of them as first-rate lives," does not appear to us to carry much weight, for it is quite possible that lives deemed free from organic disease, and altogether eligible for assurance one year, may, even before the lapse of another, be in a totally different condition; and this is much more likely to be the case when, as in one of Dr. Christison's own examples, the insurer survived not one year only, but two-thirds of his expectation term. While, therefore, disposed to accept the fact, because established by post mortem examinations, we demur to the argument by which, in this instance, it is sought to be supported.

We pass on to the mortality from diseases of the brain and nerves. In the experience of the Scottish Widows' Fund it is here that the mortality reaches its highest rate, and in that of the Standard it is only exceeded by the deaths from infectious diseases. From diseases of the brain and nerves, there have occurred in the Widows' Fund no fewer than 150, or $21\frac{3}{4}$ of the total mortality. In the Standard, 53 have been cut short from the same causes. Apoplexy and palsy are the two most fatal diseases under this division. In the Standard, 26 deaths resulted from the former, and 15 from the latter. In the Widows' Fund, 54 and 28 respectively. In combining the experience of both of his investigations in regard to the period of life when apoplexy and palsy occur, Dr. Begbie finds the statistics of Rochoux, so frequently quoted, amply confirmed. He thus analyses the results: Of 154 deaths, 37 only took

place before 50, and 117 after that age. Nearly one-third of the whole occurred between 60 and 70, and twice as many between 60 and 70 as between 70 and 80. While quite disposed to agree with Dr. Christison in the remark that much is still left for medicine to accomplish, in order to determine what is the organization, and what the other circumstances favourable to the development of the diseases of the brain, particularly apoplexy, we think that already we possess, to a certain extent, these means, and that, from investigations such as his own and Dr. Begbie's, we are likely soon to make them more useful and perfect. It is impossible to exaggerate the importance of the connexion of cardiac with cerebral disease, particularly of the apoplectic nature. And it is also too well known that apoplexy finds its victims in persons who exhibit in many ways a marked proclivity to cerebral affections; and very frequently in hereditary descent, and at exactly the same period of life as a father or progenitor has been cut off.

"Among the victims of apoplexy and palsy, as well as of disease of the brain in general," writes Dr. Begbie, "who have, during the last seven years, become claims on the benefits of the Widows' Fund, there were many of whom it was known, previous to admission, that they inherited a predisposition to these cerebral diseases; of many, that they had been affected with rheumatic fever; and of others, that they had suffered from, or were predisposed to, gout. . . . . The influence of gout and acute rheumatism on the heart and great blood-vessels, and the injurious effects of intemperance, as aiding and engendering a predisposition to cerebral disease, is still a subject too little considered in conducting the business of life assurance. . . . . Apoplexy and palsy are the two diseases of the class which it becomes the directors and medical referees carefully to study and guard against, and there is reason to hope that the same strictness of rule, applied to the examination and admission of those suspected of tubercular disease of the chest, when brought to bear on those inheriting predispositions to affections of the head, may lead to a corresponding diminution in the rate of mortality." (p. 11.)

In regard to the rapidity of the fatal issue in cases of apoplectic or hemiplegic seizure, the following interesting facts are elicited in the experience of the Widows' Fund. Of 50 cases, death took place in 25, or in one half, within the first twenty-four hours; while in 19 of the 25 it occurred within the first twelve. In the remaining 25 cases, the fatal event took place from the second to the twenty-first day after the occurrence of the apoplectic attack, the largest number of deaths being on the second and third day.

In speaking of the mortality from diseases of the respiratory organs, Dr. Begbie notices a reduction in the experience of the Widows' Fund as compared with that of the former investigation. In 1847, the mortality from chest affections amounted to 23¾ per cent. of the total loss; in the present report, it is found to be reduced to 18¾ per cent. This change is proved to be chiefly due to the diminished mortality from consumption, and is, in all probability, the result of the care and caution exercised by the directors in the selection of lives as far as possible free from consumptive taints, and to the rejection, as ineligible, of all the younger applicants of assurance in whose immediate family tubercular disease has unequivocally manifested itself. Of the 690 deaths, 42 occurred from consumption—that is, 6 per cent. of the gross mortality; of these 42, 6 died between 20 and 30; 12 between 30 and 40; 10 between 40 and 50; 10 between 50 and 60; 3

between 60 and 70; and 1 at the unusual age of 73. In the experience of the Standard, of the 293 deaths, 29 were caused by consumption. In speaking of consumption, Dr. Christison separates it from the affections of the respiratory system, and includes it, with cancer, under the head of diseases of depraved constitutional habits. Of the 29 deaths, 1 fell between 20 and 30; 16 between 30 and 40; 4 between 40 and 50; 6 between 50 and 60; and 2 after 60. Hence 17 deaths took place under 40 years, and no fewer than 21 under 50. Consumption, perhaps, of all the diseases from which selected lives are known to suffer, is the one to which most attention has, in connexion with our present subject, been directed. It is, also, a disease which all medical men who have had much to do with the selection of lives for assurance, and both authors of the papers under review, will be agreed in regarding as one from whose risks life offices are now, in no small degree, relieved. This is, of course, owing to the circumstance to which Dr. Christison thus alludes: "There can be no doubt that much greater loss has been saved by the vigilance of the directors and their officers in avoiding consumptive risks." This might almost be rendered in different words by expressing the advantages which have accrued to assurance offices by the careful examination into the family history of every applicant, now practised by the medical officers of all life offices, but first acted on by the Scottish Widows' Fund. In the case of consumption, the value of such inquiries is amply attested by Dr. Christison; but it appears to us that, in almost all cases, the same diligent and careful investigation of family history and hereditary predisposition is called for. It is, in a word, the prevailing diathesis in the family of a proposer for life assurance which must be taken chiefly into consideration; the party proposing may, in his own person, have exhibited no marked peculiarity of habit of body, or tendency to disorder of system; but it may—indeed, undoubtedly will—lead to a more accurate estimate of his probable longevity, and immunity from, or proclivity to, disease, if the family history of his nearer relatives be inquired into. Such a one, perfectly free from recognisable disease or disorder at the usual youthful period of first effecting a life assurance, may have lost a father, or mother, grandfather, or grandmother, or other near relative, at a maturer age, of gout, or of pleurisy, or of carditis intimately connected with the gouty or arthritic poison, the knowledge of whose existence in his own system is thus alone disclosed, and from which cause the insurer's life is not unlikely, at something of the same period of life, to be terminated. This, we firmly believe, is not carrying the case too far. Dr. Christison says: "Physicians know little of the constitutional and other circumstances which predispose to pleurisy." We do, however, know something of these; for—not to multiply examples—that peculiar disordered condition of the constitution which results in degeneration of the kidney, or Bright's disease, leads to pleurisy—frequently most severe, and often fatal—and rheumatism and gout, and other allied disorders, lead to both, so as to convince us that, if ever the "little" we do know is to become more, and to prove valuable in the selection of lives for assurance, it will be by the study of the diathesis of disease, if we may use the expression, and of its hereditary transmission. The experience of the Widows' Fund, in regard to the periods of life at which phthisis proves fatal, is thus referred to by Dr. Begbie in his first report:

"In the experience of the Society, the largest number of deaths took place between the thirtieth and fortieth years: 9 occurred between 20 and 30; 35 between 30 and 40; 16 between 40 and 50; 7 between 50 and 60; and 5 between 60 and 70. The period between 30 and 40, including both, numbers 37 deaths, or more than a half of the total mortality; and the age of 39 alone numbers 9 deaths, the largest in any one year, and equal to the first of the decennial periods, and exceeding considerably either of the two last." (p. 11.)

Commenting on this experience, Dr. Christison remarks:

"Of 72 deaths from consumption, after the age of 20, 22·2 per cent. occurred between 41 and 50, 9·7 between 51 and 60, and 7·0 above 60—that is, 38·9 per cent. above 40, and 16·7 above 50. The experience of the Standard Life Assurance Company during the last five years is to the same purport:—Of 29 deaths from consumption, 13·8 per cent. occurred between 41 and 50, 20·7 between 51 and 60, and 7 above 60—that is, 41·5 per cent. occurred above 40, and 27·7 above 50." (p. 43.)

These statements appear to us both of interest and value; they correct the statistics furnished by the Equitable Society from what must have been faulty data—for though phthisis does often occur after the age of 40, it is not so alarmingly common between that period and 80 as these statistics would lead us to believe. On the other hand, the statements of Drs. Christison and Begbie confirm, in great measure, those of other authors; and being founded on carefully examined data, and with an earnest desire after truth, they afford us a starting point for other and still more extended investigations.

From diseases of the heart and great bloodvessels, there were 21 emerged risks of the 293 in the experience of the Standard; and of the 690 in that of the Widows' Fund, there were no less than 66, or 9½ per cent. of the whole. Of these 66 deaths, 11 were caused by aneurism, and 55 by disease of the heart itself. Combining the experiences of both offices, 6 deaths occurred before 40, 17 between 40 and 50, 31 between 50 and 60, 27 between 60 and 70, and after 70, 8. "The morbid alterations of the heart chiefly noticed," says Dr. Begbie, "are hypertrophy or enlargement principally of the left ventricle; dilatation chiefly of the right chambers; valvular imperfection of the mitral and aortic orifices; ossification of the coronary vessels; and fatty degeneration of the muscular fibre of the heart." Both authors are particular in directing attention to the connexion of acute rheumatism with cardiac disease; a subject of great importance in the business of life assurance, to which Dr. Begbie drew the notice of the directors of offices very fully in his first report; and which is now beginning to be by them thoroughly appreciated. We cannot agree with Dr. Christison, when he says, as the result of his own experience in the Royal Infirmary, as well as in private practice, that "rheumatism is not so very often associated with inflammation of the pericardium and endocardium here, as it undoubtedly is in London, by Dr. Latham's showing." We have before us an account of 16 cases of acute rheumatism, admitted into the Royal Infirmary of Edinburgh during a period of eight months, and of these no less than 10 suffered from the extension of the rheumatic inflammation to the heart. The question raised, however, by Dr. Christison is a very important one; and it is not unlikely that the statistics of insurance offices may hereafter prove valuable in enabling us to determine, whether or not in certain

situations or localities, the heart is less likely to become affected during rheumatic fever than in others. The experience of the Widows' Fund and Standard differs on the point in question. Of 53 persons assured by the Widows' Fund, who died of diseased heart, there were 13 who had suffered from acute rheumatism before acceptance; whereas in 17, of what are called adequately recorded deaths in the Standard's investigation, only 2 occurred in persons ascertained to have suffered from rheumatism.

Diseases of the organs of digestion is another class from which insured lives suffer in large proportion; and it is one, writes Dr. Begbie, "which calls for particular attention on the part of the directors of life assurance societies; and there cannot be a doubt that, by instituting a more rigid inquiry into the habits and mode of living of the parties proposing insurance, the number of deaths might be materially reduced. The diseases of the organs of digestion are especially those of intemperance, both as regards eating and drinking; and a careful examination of the documents in the possession of the society, shows that too many have been admitted on the ground of their good health, while their habits and mode of living might have given rise to serious doubt how long this was likely to be preserved." (p 21.) These sentences appear to us alike pregnant with important truths, and suggestive of what must prove to be a better and a fairer discrimination and acceptance of lives. The grounds indicated in the passage now quoted, for the rejection of otherwise eligible lives, are most righteous; for while we are, on the one hand, entirely opposed to the narrowing and exclusive system in insurance, of which we see some symptoms; and while we feel deeply for many lives which are wisely, because necessarily, rejected from the benefits of life assurance, at least, on ordinary terms—we confess to feeling no sympathy for that other class, who may have been accepted, but would have been righteously excluded—whose disease, though of course inflicted by a higher hand, is undoubtedly due, in great measure, to their own folly and sin. Insurance of life for mutual benefit has always appeared to us, if we may be allowed the expression, a great moral mistress; and we have little doubt, that over all well-regulated minds it has a moral weight, and by those persons who share in it, it is so regarded. Once insured, a man is bound, not by personal or family motives merely, but by the even higher claims of being answerable to other and fellow-sharers in insurance benefits, to maintain his bodily health in as high a state of vigour and excellence as possible, and so to avoid all excesses *which will infallibly lead to* a different result. Thus, often reflecting, we have conceived the idea that insurance offices, established to meet the wants of persons lower in the social scale than those assurance presently benefits, might take the place, in certain cases, at least, of temperance or total abstinence associations. We want assurance offices for the lower orders; and we desiderate the total destruction of burial societies. Any man who is convinced of the folly and sin of intemperance, and has sufficient moral vigour and courage left to determine on a wiser and better course of life, might effect an insurance in such an office as we have indicated; and his premium paid weekly or monthly, while the tangible proof of his own reformation, would stimulate and encourage him.

The chief diseases included under the head of Organs of Digestion, are those of the Stomach, Bowels, and Liver. They are thus tabulated:—In the Standard—Organic disease of the stomach, 11; of the liver, 11; dysentery and diarrhœa, 11; obstruction of the bowels, 1. Of diseases of the stomach and bowels there were, in the Widows' Fund, 34; and of the liver, 34; the large mortality being between the ages of 50 and 60. The only other source of mortality to which we shall allude, is that from disease of the urinary organs, which, in the experience of the Widows' Fund, amounts to 30, or 4½ per cent. of the whole; in that of the Standard, to 8 deaths only. It is probable that the per centage of deaths in both offices would have been raised—certainly so in the Standard—had there been a nicer and more truthful distinction of the dropsical cases which occurred; and several of which, from want of more certain information, are placed under the unmeaning title of Diseases of Doubtful Seat.

We must here bring our remarks to a close, having referred, as proposed, to some of the more frequent causes of mortality, as given by both authors. We might continue our observations, and with great pleasure and propriety transfer to our pages many of the interesting and valuable remarks with which, in both pamphlets, the details of the fatal causes are accompanied; but as both are easily obtainable, we prefer recommending their attentive perusal to our readers.

The subject of the mortality of insured lives, viewed in both aspects—medically, and as affecting the business of life assurance—is one for which much still remains to be done; but access to the hidden stores of useful information has been obtained; the Equitable, the Scottish Widows' Fund, and the Standard, in the persons of their directors and medical advisers, merit our warmest thanks. We shall look for further insight from the extended investigations of the latter; and we shall also look for fellow-labourers with them in the same rich and extensive field; for, placed in such responsible and honourable, but desirable, positions, our profession has a right to expect a harvest from their labours.*

Review IX.

Observations on the Structure and Development of Bone. By John Tomes, F.R.S., Surgeon-Dentist to the Middlesex Hospital; and Campbell de Morgan, Surgeon to the Middlesex Hospital. ('Philosophical Transactions,' 1853.)

No part of physiology has undergone a more complete revolution, within the memory of man, than that which treats of absorption. Five-and-twenty years ago the Hunterian doctrines reigned supreme. Hunter, whose very errors have a certain grandeur, had propounded a most com-

* We beg to direct the attention of those interested in this subject to two very well considered letters on the medical department of life assurance by Horace Dobell, Esq., the second of which particularly contains many suggestive remarks, in which we entirely concur. Also, to a very valuable pamphlet from the pen of the late lamented staff surgeon Dr. Henry Marshall, entitled, 'Memorandum for the procedure for effecting a Life Assurance, with Observations on some of the points which require to be considered by the Directors of an Association for the Assurance of Lives.'

prehensive scheme, in which the power of absorption, in every possible degree and variety, was attributed to the lymphatic vessels. These he supposed to possess the function, not merely of introducing new material into the animal body, and of removing the old, but likewise of modelling and giving the finishing touch to all organic forms; for it was assumed that the various tissues were first roughly cast, as it were, by the blood-vessels, and then pruned of all excrescences and superfluities by the absorbents. And not merely so, but the removal of tissues before an increasing abscess or aneurism was ascribed to the same agents; ulcers were considered the work of the devouring "mouths of the absorbents;" and the formation of ulcers in bone and cartilage was taken as an irrefragable proof that those tissues must possess these vessels, though so small as to defy the most microscopic search.

The first inroad on the established belief was made by the investigations begun by Magendie, and carried on by various observers down to Liebig, into the fact of imbibition by the bloodvessels; and by the knowledge, gradually gained, of the circumstances regarding the fulness of the vessels and the composition of the blood, which cause exudation from, or absorption by, the vessels, as the case may be.

Then came a new phase of opinion. The observant and practical surgeon, Aston Key, was led, by what he saw in the processes of disease, boldly to reject the Hunterian hypothesis, and to declare that ulceration was not an absorption, but a physical degeneration and liquefaction; except that, in some instances, ulceration of cartilage was effected by means of a false membrane creeping over its surface, which he believed to have the power of absorbing the cartilage; or by means of granulations springing from the bone, which he supposed able to eat through it from its attached surface.

We could, did our space permit, give an amusing account of the opposition which Key's doctrines met with at the hands of physiologists, so thoroughly imbued with Hunterian doctrines that they were absolutely incapable of comprehending any vital process whatever in which *action of vessels* was not concerned.

But these quarrels were soon forgotten in that greatest change of all—when physiologists began to scrutinize the structure and vital processes of tissues themselves, and when cell-growth took the place of organization by means of capillaries, and modelling by absorbents. Amongst the earliest workers in the microscopic school, Goodsir soon began to investigate the processes of ulceration and absorption; and arguing from the phenomena of the absorption of chyle and lymph, he adopted and worked out Key's theory of the absorption of cartilage by means of a vascular membrane, illustrating it by the light of the cell theory, and believing that he could demonstrate the *law* of the growth of one set of cells at the expense of another, and the *fact* that not only was articular cartilage absorbed by the instrumentality of an active cell-growth on its surface, living on it, and increasing as it diminished, but that all other ulcerations were caused in the same way.

Having given this rapid sketch, the relevancy of which to the matter in hand will soon appear, let us turn to Messrs. Tomes and De Morgan, and see what additions they have made to our knowledge of the structure, development, and absorption of bone.

And, following the plan of our authors, let us begin with a glance at a transverse section of the shaft of a long bone. Here we have the spectacle so familiar to our readers—the central medullary space; the numerous smaller apertures, well known as the Haversian canals for bloodvessels; the series of concentric laminæ of bone surrounding each of these canals; and the numerous small cavities, or *lacunæ*, lying amongst the laminæ, and having minute tubes, or *canaliculi*, radiating from them. Each Haversian canal, with its surrounding laminæ and lacunæ, forms a separate entity,—an Haversian system; and of such systems all bone is essentially composed: differences in density and porosity being due to the greater or less compactness with which these systems are packed together, and to the size and number of the canals, not to any essential difference in structure.

But a closer inspection will reveal some other points in our transverse section that must be noticed, and for our knowledge of the real signification of which we are indebted to our authors.

For instance: a long bone is composed of a number of Haversian systems, packed together like a bundle of rods. Between these systems there must be interstices. Now, let the observer notice that these interstices are filled with bone tissue; which is not amorphous, as if merely interstitial, but is composed of patches of laminæ, curved and parallel, but broken and fragmentary. Now, how is the presence of these *interstitial laminæ*, as they are called, to be explained? It may be seen that they are not related to, or continuous, or conformable (as the geologists would say) with the strata of the adjoining Haversian systems; but their regular parallel curves show them to have some relation, not obvious at first sight. What is this relation?

Then, again, if any one Haversian system be closely inspected, it will be seen that its innermost ring, forming the canal for the vessels, is smooth and oval; but that its outer ring is more or less waved and irregular in outline, fitting in, like a piece of a dissected map for children, between the irregular patches of interstitial laminæ by which it is surrounded.

Further than this, a careful scrutiny of the section will reveal that, in addition to the smooth oval apertures of the Haversian canals, each of which is surrounded by one unbroken lamina, there are other spaces, widely different in character. These (which our authors propose to call *Haversian spaces*) are irregular—jagged in their outline; which outline, instead of being formed by one smooth unbroken lamina, is formed by the irregular and broken edges of several Haversian systems of laminæ.

But it must be evident, that if any one entire Haversian system were taken clean away from out of the irregular patches of broken laminæ which surround it, the empty space left would be exactly similar to, or more properly, exactly identical with, one of those spaces which we have described as Haversian spaces.

If these points be borne in mind, our readers will be prepared for the explanation which our authors have given of them, which is this: that bone is not a thing of permanent substance, but that portions of it are incessantly disappearing, leaving the irregular spaces which our authors have designated Haversian spaces; that these spaces so formed are filled

up with new Haversian systems; whilst the remains of the older systems are found interspersed amongst the new ones, in the form of the interstitial laminæ.

"This," say our authors, "is a very important fact, as it demonstrates that the old tissue is removed in masses, and a new one developed in its place. It has long been taught that the older particles of an internal tissue are removed by absorption, and new ones substituted, and this throughout the life of the individual. But the authors believe that they have demonstrated it for the first time."

We shall return to this part of the subject hereafter; for the present, having briefly sketched the Haversian systems, and our authors' views of their vital history, we may complete our survey of the parts visible in a section of bone, by noting the ultimate structure of bone tissue: which is not, as is sometimes described, fibrous, but is composed of granules or granular cells, imbedded in a more or less clear homogeneous matrix; the laminæ in which the tissue is arranged being constituted by an alternation of the granular with the transparent structureless material; and lastly, that the circumferential laminæ, which are usually described as forming the surface of bone, are entirely absent in the bones of fast growing animals, and only present in those of adults, whose bones have attained their full diameter, and in those of young individuals whose growth from any cause is arrested.

Having thus plunged *in medias res*, let us now go back, and, as briefly as is consistent with intelligibility, relate *ab ævo* what our authors tell us of the processes of development by which bone attains its perfect condition. And here we meet with an instance not uncommon in the history of development, of an end attained by two different series of means; or of one process employed for the original formation, another for the increase, extension, and renewal of a tissue. For bone may be developed in either of two forms: either through the medium of cartilage, or of osteal cells. Bone in general is first formed, and cylindrical bones increase in length, through the medium of cartilage; but the flat cranial b[illegible] increase in size, the cylindrical increase in breadth, and all receive re[illegible] of their tissue internally, by means of the osteal and lacu[illegible] cell[illegible] us describe the cartilaginous process first.

"Temporary cartilage," say our authors, "when it first appears in the embryo, consists of an aggregation of closely-packed nucleated cells, which in the process of growth become separated by the development of a tissue external to them, usually designated the hyaline tissue of cartilage."

The next step seems to be the fusion of the outer wall of the cartilage cell with the hyaline tissue, and the conversion of the original nucleus into a granular cell, with one or more nuclei. In making sections of cartilage, many of these granular cells escape from their cavities in the hyaline tissue, and may be seen detached and floating about in the field of the microscope. Next ensues a rapid growth, principally in the direction of the long axis of the future bone. "Each granular cell becomes divided into two, by segmentation transverse to the line of ossific advance." These cells are again divided, and the process repeated from time to time, till in the place of a single granular cell, we have a long line of cells, extending from the unchanged cartilage, to the point where ossification is taking place. Contemporaneously with this development

of lines of cells, other changes are going on in the individual cells composing them. If we examine those situated near the advancing bone, it will be observed that they have enlarged, have become separated from each other by wide intervals, and that each is surrounded with a thick pellucid cell-wall. The increase in the size of the cells has occurred at the expense of the hyaline tissue, which at these points where the rounded cells approach each other, is reduced to a thin film. Here, then, we have a hyaline matrix, containing cells composed of three parts—an outer pellucid cell, a granular cell, and in this one or more nuclei.

Thus, then, the preparatory changes in temporary cartilage previous to ossification, consist in its rapid growth, in the production from single cells of long lines or columns of cells, and in the enlargement of, and formation of a pellucid wall around, each individual cell. On this last point, say our authors:

"In examining a line of these bodies, extending from the forming bone of the diaphysis, we shall see them in various degrees of forwardness. Thus, if attention be directed to the end of the line furthest from the bone, the cells will be found small in size, granular, and with a perceptible nucleus: but they have not an outer wall distinguishable from the hyaline substance, which is abundant between the contiguous lines, but small in quantity between the cells composing the lines. But if the other end of the line be examined, very different conditions will be observed. The granular cells will be seen to have become rounded in form; to have increased to three times their original bulk, and to possess well-marked circular nuclei; in addition to which, each granular cell will have acquired a thick pellucid outer wall, while the hyaline tissue between contiguous lines of cells will have dwindled down to a thin film, except in those parts where spaces are necessarily left in the approximation of spherical bodies."

So far, then, we have changes preparatory to ossification. Now for ossification itself. First, the hyaline or intercellular tissue, both that which lies between the columns of cells, and likewise that which passes more or less perfectly between individual cells, "becomes in some cases slightly fibrous in appearance, and of a light-brown colour; this condition speedily gives way to a highly granular state; in fact, it has become bone." Thus, then, the intercellular substance has become bone, and forms little bony crypts, containing the cells, which, on making thin sections, and examining them in water, may occasionally be seen detached from the crypts, and floating in the field of the microscope. The next step is, that these cells (henceforth to be called *lacunal cells*) themselves ossify. First, the outer pellucid coat displays a few granules on its surface; the granular cell is seen distinctly within it, minute processes or elongations beginning to project from its surface, and the nucleus is also seen distinctly. Next, the outer pellucid coat becomes more distinctly granular, the enclosed cell decidedly angular or ragged, and the nucleus is obscured. Next, as ossification advances, the granular cell, with its processes, becomes united to the outer coat, so that they cannot be separated the one from the other, and are no longer to be recognised as distinct parts. "If accidentally broken across, we see that they have a hollow centre, in fact a *lacuna;* but when loose and entire, they appear as rounded dense masses, projecting from the surface of which, we may not unfrequently detect a few short needle-like processes." Lastly, the lacunal cells, thus ossified, unite with the previously ossified intercolumnar tissue, from which they are henceforth inseparable.

These various steps of the ossifying process may be very favourably seen by making a transverse section through the ossifying parts. First, the intercellular tissue which has become bony, will present itself in the form of more or less perfect septa, lying between and enclosing the lacunal cells. At a part rather more advanced, the lacunal cells will be seen becoming granular at their circumference, whilst the granular cell (the future lacuna) is seen within, with its nucleus distinct, and with numerous processes (the future canaliculi) extending from the circumference to the surface of the outer cell-wall. Again, if another section be taken at the point where ossification is just completed, the various points described as existing in ossifying cartilage will be seen in that which is converted into bone. Thus, we shall see the intercellular tissue preserving its original form, and highly granular; and that, while the outer walls of the lacunal cells have become calcified, the granular cells will have assumed the form of perfect lacunæ and canaliculi, the latter freely intercommunicating when the surface of the lacunal cells is in contact, but seldom extending into the ossified intercellular tissue.

The successive steps of this process may be thus briefly summed up: Formation of cartilage cells; formation of intercellular substance; fusion of cartilage cell-wall with intercellular substance; development of the nucleus of the cartilage cell into a granular nucleated cell; multiplication of these granular cells in linear series; enlargement of them, and formation of a thick pellucid outer coat around them; ossification of the intercellular substance; ossification of the pellucid outer coat of the cell; development of the granular cell into a lacuna, with projecting canaliculi; union of the ossified cell-wall with the intercellular substance.

Bone, thus formed by the calcification of cartilage which has previously assumed the structural arrangements of bone, is called *primary*, and, we shall presently show, is destined to no long existence, but is soon excavated by absorption into channels in which Haversian systems are developed: but we must return to this point after having spoken of the second mode of bone development—that, namely, by osteal cells; by which latter process, the flat bones increase in breadth, and the cylindrical in diameter, and by which Haversian systems of laminæ are formed within the substance of all bone.

"If the advancing edge of a parietal bone be taken either from a human fœtus or from a fœtal lamb, and the pericranium and dura mater be carefully removed from their respective surfaces, we shall find the growing bone still invested with soft tissue on the inner and outer surface, which is prolonged from the free edge. When examined under a favourable light, this tissue will show differences of character in different parts, varying with the distance from the bone at which the observations are made. Thus, if attention be directed to the part farthest removed from the bone, it will be seen that the membrane-like mass is composed of oval cells, with slight prolongations from the extremities, which are frequently arranged in [illegible]m of bands of fibrous tissue. 'Dr. Sharpey has observed, that the mem[illegible]to which the bone extends is exactly like fibrous tissue in an early stage [illegible]lopment; and this observation is strictly true when confined to the part [illegible]icated, but the analogy ceases as we extend our examination towards the bone. Here, [illegible] the place of cells with elongated processes, or cells arranged in fibre-like lines, [illegible] find cells aggregated into a mass, and so closely packed as to leave little ro[illegible] intermediate tissue. The cells appear to have increased in size at the [illegible] the processes which existed at an earlier stage of development, and formed

a band of union between them. Everywhere about growing bone, a careful examination will reveal cells attached to its surface, while the surface of the bone itself will present a series of similar bodies ossified. To these we propose to give the name of *osteal cells*, as distinguished from lacunal and other cells.

"In microscopic characters, the osteal cells closely resemble the granular cells of temporary cartilage; so closely, indeed, that the latter, when detached from the cartilage, could not well be distinguished from them. They are, for the most part, spherical or oval in form, and lie on the surface of the growing bone in a crowded mass, held together by an intervening and apparently structureless matrix. Here and there we find a cell which has accumulated about itself an outer investment of transparent tissue, and has, in fact, become developed into a lacunal cell, destined to become a lacuna.

"The process of growth may be thus described: In the meshes of the fibrous tissue, on the surface of the bone, osteal cells are developed, and gradually take its place; a few cells become developed into lacunal cells; the earthy salts are added, and, concurrently, lacunæ and canaliculi are formed. We, then, have bone presenting the usual characters of that tissue.

"The process by which cylindrical bones are increased in diameter is, in all respects, similar. Osteal and lacunal cells are present, but the relative amount of matrix is greater; moreover, the osteal cells have a disposition to assume a linear arrangement, corresponding to the direction of the laminæ of the contiguous bone. In these lines, the cells are placed so close to each other as to leave but little room for intervening tissue; but between the lines, an appreciable amount may be recognised. This appearance, however, varies in different specimens. In one, the cells predominate; in another, the transparent tissue is the more abundant. Generally, the younger the animal, the greater will be the amount of the intervening transparent tissue, and the smaller the number of the osteal cells."

Through either of these processes, then, bone may be developed. On the *teleological* or *final-cause* question, as to the special purpose answered by either mode of development, the authors believe that it may be stated thus: The temporary cartilage previous to the development of bone affords a mechanical support and protection to the soft tissues contained within or lying around it. These offices could not be rendered by a mere mass of soft osteal cells.

"A second and scarcely less important purpose, effected by temporary cartilage, is that of affording a medium for which a more solid tissue may be substituted, without the mechanical support being withdrawn from the adjoining parts during the process of change It affords, also, a means by which the long bones are gradually increased in length, without any interference with the functions of the limb. These changes are brought about by the gradual increase in the number of the cartilage cells, at those points only where ossification is about to commence; and by the conversion of the cells into lacunal cells, at the cost of the intercellular tissue, which, while its bulk is diminishing, becomes impregnated with the earthy salts; so that, although the quantity is lessened, the strength of that which remains is increased."

It may suffice, however, to sum up this part of the subject with the statement, that wherever mechanical firmness is desirable, before or during the development of bone, there we have temporary cartilage; wherever this is unnecessary, we have the osteal cells.

But, as we have before said, bone, however formed, is a thing of no permanent existence; but is subject to incessant absorption and renewal. Even here, however, there is a striking difference between the *primary bone*, or calcified cartilage, and that derived from osteal cells. The former, which is, as it were, a solid block of material, very speedily becomes per-

forated and channelled into Haversian spaces, which are necessary in order to permit the adit of bloodvessels, and in order to allow material to be brought for the calcification of successive strata of cartilage; for true bone seems always to require a good supply of red blood in its vicinity. On the other hand, the bone which is developed through osteal cells already contains the bloodvessels of the fibrous tissue, in which the cells were formed. The surface of a young and growing bone, moreover, instead of being smoothly circumscribed by a circumferential lamina, is perpetually "sending off outrunning processes between those vessels of the periosteum which lie nearest the surface of the bone;" which processes increase, bifurcate, arch over, and enclose the vessels, and send out fresh processes, enclosing fresh vessels which become the centres of Haversian systems: so that the latter kind of bone does not require to be immediately channelled out for the introduction of bloodvessels, as the former does; yet, in process of time, it is perforated equally with the other; and those longitudinal channels are formed, the section of which we took notice of in a former part of this paper under the name of *Haversian spaces.*

The Haversian spaces, then, are longitudinal cavities tunnelled out of the substance of bone, and are evidences of a constant process of removal or absorption of tissue. They are, as we before said, irregular in their parietes; cut out apparently at random, and having the cut edges of numerous laminæ at their circumference. They are, as might be surmised, most numerous and large in young and rapidly-growing bone, especially in the *primary bone,* which, we need not repeat, is the recently calcified cartilage; in old bone, they are less numerous, but never entirely absent.

Concurrently with the disappearance of tissue by the formation of Haversian spaces, is the reproduction of new tissue, by which these spaces are filled with new Haversian systems of concentric laminæ. This our authors shall describe in their own words:

"The manner in which the Haversian spaces become gradually occupied by Haversian systems is peculiarly interesting. To obtain a good view of the process, it is necessary to make a transverse section of the developing systems; it may then be seen that osteal cells arrange themselves in single file within the Haversian space, with intermediate lines of transparent tissue, and here and there a lacunal cell; the process commencing at the surface of the Haversian space, and extending gradually inwards till the system is completed. 'In fact, the soft tissue takes the permanent form previous to the addition of the salts of bone, much in the same manner, and to the same degree, as occurs in temporary cartilage before the earthy ingredients are deposited. Lamination is nothing more than a definite linear arrangement of the osteal cells, with their outlines permanently retained in the perfected bone;' a character much more strongly marked in the bones of adult than in those of young animals."

It must be added, that the production of new Haversian systems, like the process for the formation of Haversian spaces, is more active in young than in old subjects; but the authors have noticed it in the bones of those who have passed their sixtieth year.

We hope that the foregoing remarks, which have cost us no small pains to elaborate with even the slightest pretension to clearness, will be tolerably comprehensible to such of our readers as will take the trouble to examine for themselves a piece of ossifying cartilage, and such sections of bone as

they may be able to obtain. We must quit this part of the subject, with one word about the *lacunæ*, or *bone corpuscles*, as they are sometimes called. Respecting these, it seems established by our authors, that they are real cells, having persistent nuclei and real cell walls; and that the canaliculi are minute tubes passing off from these cells, and having also real parietes; that the lacunæ originate in granular cells; either those of temporary cartilage (if the bone be *primary*), or in certain ones scattered at equal distances amongst the osteal cells, which have been designated lacunal cells, and which are distinguished at an early period by the accumulation of a greater quantity of transparent substance around them, in the form of an outer coat, which afterwards ossifies, and adheres firmly to the tissues around. Meanwhile, when a portion of bone attains its highest development, its canaliculi are numerous, passing chiefly across the laminæ towards the Haversian canal; but likewise forming rich anastomoses with the neighbouring lacunæ, and with those of adjoining Haversian systems.

But to return from this digression. We have hitherto been treating of matters of fact, which admit of perfect demonstration. Now we have to approach matters of inference, with regard to the mechanism by which the absorption of bone generally, and the formation of the Haversian spaces in particular, is effected. Our authors' views are essentially those of Key and Goodsir; but we will let them describe them in their own words.

"During the present winter, it became necessary to remove a portion of the femur which protruded from a stump six weeks after the removal of the limb. From the medullary cavity a granulating mass projected, and covered the surface of the bone left by the saw; and as the bone was rapidly wasting from the inner or medullary surface, we had in this specimen a favourable opportunity of examining the tissue which lay in immediate contact with the surface of the wasting bone. On cutting through this piece of femur in its length, with a very fine jeweller's saw, it was found that a dense pale pink tissue lay in contact with the inner surface of the bone, which was hollowed with numerous minute cavities, into which the soft tissue accurately fitted, but from which it could be detached without tearing. The outer surface of the bone had been deprived of membrane many days before its removal from the limb.

"The examination of the tissue thus applied to the fast-wasting bone, offered as favourable an opportunity for learning something of the means by which absorption is effected, as we could reasonably hope to obtain; the more so, since the outer surface having been for some time exposed, and covered only by dried periosteum, the actions had been confined to the inner surface of the bone. A careful examination showed that the surface of this tissue was composed of minutely granular nucleated cells, which lay in close and immediate contact with the bone, and increased in an exact ratio with its diminution. What the bones lost in bulk the cells gained, the cellular mass presenting a perfect cast of the surface of the bone; suggesting to the mind that the soft was growing at the cost of the hard tissue, or, at all events, that the former was instrumental in the removal of the latter. The cellular mass was tolerably vascular, but the vessels did not reach the surface in contact with the bone; hence they could not be regarded as having any immediate action in the process of absorption. Section of the bone showed that the medullary cavity had been greatly enlarged by absorption; and no doubt, had sufficient time been allowed, the femur at that part would have been reduced to a thin scale. A transverse section showed, that in many, though not in all instances, the Haversian canals had been enlarged, and rendered irregular in shape,

but it was evident that the process of removal had been less active in this situation than on the medullary surface of the bone.

"If we examine the fangs of temporary teeth, when they are undergoing removal, similar states to those described as existing in the portion of femur will be found to obtain. A similar cellular mass will be seen to be closely applied to that surface of the tooth which is in process of removal, and the surface itself will present the characteristic emargination observed in the bone. When we connect these conditions with the fact, that the nucleated cells which form the embryo have the power of appropriating the material which lies about them to the purpose of their growth and their conversion into the various animal tissues, it is difficult to resist the belief, that the cells which lie in contact with wasting bone and dentine, take up those tissues and use all or part of their element for the purposes of their own increase or multiplication, or else form a medium through which they are passed into the circulation.

"But as the process of absorption, with concurrent development of cells, is most active in primary bone, where but few vessels exist, the former hypothesis seems the more probable. An objection may be raised to the supposition, that the bone is absorbed by the cells, on the ground of the density of the former; but it must be borne in mind, that as the density is gradually imparted to the bone through the agency of the adjoining soft parts, there seems no good reason for disbelieving that they may also be instrumental in its removal."

The authors have not been able to discover any distinguishing marks between the absorbent cells and those by which the new bone is constructed, although, when *in situ*, the developmental cells may usually be known from the absorbent, by the presence of lacunal cells amongst them, and by the fact that the Haversian space in which they are found, is also, by their means, losing its ragged and eaten appearance. The situations in which absorbent cells arise, are either beneath the periosteum, or beneath the medullary membrane, or within an Haversian canal; but their mode of origin, their measure of increase, the reason why, after a certain time, they cease their destructive, and commence their reconstructive, functions, are at present inscrutable. Moreover, in the case of the deciduous teeth, the absorbing organ commences within the periosteum which covers the fang, absorbs this part so as to come in contact with the pulp, which then *assumes the same function*, and grows, absorbing as it grows, till little besides the enamel is left. When this mere residuary shell is removed, the hitherto devouring organ becomes covered with epithelium, and assumes the structure and appearance of the surrounding gum.

In conclusion, the authors state their belief, that ivory pegs driven into bone, for the purpose of causing the reparation of ununited fracture, may be attacked and eroded by the absorbent cells, just like the fangs of deciduous teeth, or parts of the living bone. If we were to sum up their views on this part of the subject, they would probably amount to this,—that bone and tooth, living or dead, may be absorbed by a cell-growth developed within or upon them; that these absorbent cells have no special characters, and that they may be afterwards developed into bone, fibrous tissue, or epithelium; and that the growth of one tissue at the expense of another is a sort of general law.

Now, in treating of absorption, we must make a distinction, for there are few terms more loosely applied. For instance, there is the so-called absorption of all the tissues, which takes place during periods of great bodily exertion, mental anxiety, and insufficiency of food; the wasting

of a paralyzed limb;—the formation in the fœtus of various natural apertures;—these are not instances of absorption, properly so called, but of waste ill repaired, or of a ceasing to live: there is absorption, it is true, but incidental, secondary, or passive; a part having first ceased to live, its molecules are removed, and are not replaced. The efficacy of pressure in "stimulating the mouths of the absorbents" seems to consist, in most cases, in its mechanical property of hindering stagnation or distention of the veins; but in others it may interfere with the life of the part pressed on, and cause it to waste, and, once wasted, not to be replaced. Drugs seem to owe whatever efficacy they possess in promoting absorption to two causes. Sometimes such medicines as were once called deobstruents, but now catalytics, seem incompatible with the life of some morbid growths, and cause them to waste; in other instances, they deprive the blood of a certain ingredient, which it then more readily takes to itself, if it meets with it in the course of the circulation. But, with the exception of the few cases in which a solvent or exhaurient drug causes absorption, the use of the term, so far as it implies an active, primary, substantive process, is a mistake; for most instances of absorption are not cases of a destructive, but of the failure of a reconstructive process; not of the operation of anything external to the part disappearing, but of its own inherent laws of life.

What shall we say, then, to the fact revealed by our authors, of the formation of the Haversian spaces? Is there a vital change in the bone preceding the cell-growth? or is the cell-growth the real, primary, efficient cause of the absorption of the bone? If what our authors have told us of the absorption of ivory pegs be true, the cell-growth must be the real agent. But can this be regarded as a general law? It may be, but we doubt if there are yet facts enough, in human anatomy at least, to prove it. The case which comes nearest to it—viz., the supposed absorption of cartilage by the agency of a cell-growth—has been clearly shown by Redfern to be a mistake. It is not the false membrane which absorbs the cartilage, but the cartilage which, by a gradual and intrinsic series of changes, becomes converted into a substance like false membrane. Ulceration of soft tissues by cell agency, as assumed by Goodsir, is a thing which we do not believe to have been ever proved. The growth of the ovum is not at the expense of the uterus. In fact, absorption—active, direct, and primary,—of a living part by the growth of another tissue, which really *takes into itself* the substance of that which it causes the removal of, seems so far as we know at present, to be confined to bone and teeth.

This extremely interesting fact, however, which our authors have unfolded, makes us thirst for more of the same sort. What is the length of life of each tissue? How fast, for example, is the brain worn and replaced?—how much faster in the hard student than in the idle? Is there a process of removal and renewal in morbid growths, as cancer? But to these, and similar questions, patient, conscientious, and intelligent research, like that of our authors, will in time find replies.

Robert Druitt.

Review X.

Reports on Epidemic Cholera. Drawn up at the desire of the Cholera Committee of the Royal College of Physicians. By William Baly, M.D., and William W. Gull, M.D., Members of the Committee.—*London*, 1854.

The fatality of cholera, and the great difference of opinion as to its prevention and appropriate treatment, gave rise, during the epidemic of 1848–9, to a very general wish that some expression of opinion should be made by the College of Physicians on these points, and that, if possible, a summary of our knowledge of cholera, which might be taken as a convenient point of departure for future inquiries, should be given by that learned body.

The Reports of the Board of Health, able as they were, were limited in scope and did not touch at all on the pathology and treatment of the developed disease, while there was an impression (an unfair one, as we think, in the case of cholera) that the Board was committed to uncertain and imperfectly proved views.

The College of Physicians, therefore, appointed a committee, who issued a series of questions to the fellows and members of the College, and to other gentlemen who were supposed to be willing to aid in the inquiry. The answers given to the questions were submitted to two members of the committee, Drs. Baly and Gull, to whom were entrusted the formation of the Report. The reporters selected for their respective treatment different parts of the subject: the phenomena attending the transmission of the disease falling to the share of Dr. Baly, while Dr. Gull undertook to report on the pathology and treatment.

It was soon found that it was necessary to extend the inquiries beyond the materials collected by the committee, although these were abundant and important, and to include all recent observations, wherever made. The Reports, therefore, have assumed the shape of a complete summary of the facts observed in the recent epidemic in Europe and America, and in addition, the records of the earlier epidemics have been in many cases referred to for illustration and corroboration. An extremely valuable work has been thus written, which may justly be compared to those elaborate and able Reports which were issued in Bengal and Madras, immediately after the tremendous epidemic of 1817.

Dr. Baly's Report is "On the Cause of Asiatic Cholera, and its Modes of Increase and Diffusion." A few words must be said in the first place on the method in which Dr. Baly has carried out the inquiry.

After getting before him all the facts elicited during the epidemic, and keeping in view, though not allowing himself to be influenced by, the various theories hitherto proposed, Dr. Baly proceeded to observe whether any generalizations at once presented themselves. Taking the map of England and the various reports on the prevalence of cholera, he saw at once that certain conclusions must be drawn. An obvious but a very important one was, that the distribution of the disease was very unequal—that is to say, that certain places were much more affected than others.

Pursuing the inquiry, it at once appeared that the districts most severely affected had, as a rule, certain common characters; yet occasionally, severely affected districts had not these characters, and other districts which did have them were not severely affected. Here then was a general rule, and two grand exceptional rules, to be investigated. After this had been done, and a vast amount of evidence brought to bear on the points, the facts were again contemplated, and other general rules presented themselves—viz., that cholera had a longer duration in a large than in a small town; that it varied in intensity during its continuance; and that after a time it altogether disappeared. Then, of course, came the questions, Why did it last longer in a large than in a small town? why did it vary in intensity? why did it disappear? &c. By the time these questions have been answered, the modes of prevalence, increase, and transmission of cholera begin to appear; and elucidated as they are by numerous authenticated facts, they assume the position of conclusions as certain as any that are known in what are rather ostentatiously called the exact sciences. The inquiry was then pursued by proposing certain questions to which answers were sought in the facts at command, such as, How far was the outbreak simultaneous in various towns? what were the periods of greatest intensity in various towns? what were the features of local outbreaks? how did the disease traverse a continent or a sea? &c.

It is apparent that not only is this the proper method of inquiry, but that the plan pursued by Dr. Baly has had the great advantage of presenting us with an investigation altogether based on original facts, and conducted entirely irrespectively of the opinions of others. The result is, that when Dr. Baly agrees with the opinions previously held—and he does this in a great number of cases—it is confirmatory testimony of the best description; testimony which we should regard in the same light as when one chemist repeats the experiments and confirms the conclusions of another.

We do not intend to follow Dr. Baly in his elaborate argument; it would be impossible to do so without giving either a dry abstract, or overloading our pages with illustrations. We shall not so logically commence *ab origine*, but shall rather ascend to the culminating point of the inquiry, and then wander here and there into the various questions which open before us.

Our readers must not expect to find that every proposition stated by Dr. Baly is novel. We have already intimated that much of the value of the Report consists in the confirmation given to previous opinions, and in the increased certainty and precision of the evidence. Of course the subject of cholera offers no virgin soil for any one; the labourers on it have been too numerous and ardent.

The cause of cholera is held by Dr. Baly, not to consist in any special atmospheric condition, but to be a material substance; an opinion which is now, we believe, almost universally received. This material substance finding its way into any place, can increase under the influence of foul and damp air, with the aid of some degree of warmth; cholera is thus "so far connected with the conditions of low site and defective sanitary provisions, that it is never rife, except when they are present in a marked degree." (p. 216.) The variations in the intensity of an epidemic are in

great part owing to accessory circumstances, which increase the dampness or foulness of the air. Among these, temperature plays an important part; but, in addition to temperature, and to all other appreciable circumstances, it is shown that there are some unknown agencies differing from the ordinary meteorological conditions, which have a powerful influence in aiding the development of the material cause.

The transmission of this material cause from place to place depends, Dr. Baly thinks, most decidedly and to a great extent on human intercourse—that is to say, men carry the disease with them in their clothes, in their ships, in their caravans, as first pointed out by Jameson, in the Bengal Report of 1824: and as since especially insisted upon by the Swedish Commissioners of 1848. At the same time, it is argued with great clearness that, although thus spreading in many cases by this agency, it does not follow that the material cause spreads by true contagion:—i.e., by reproducing itself in the bodies of men, and there only. As this is a very material distinction, we give Dr. Baly's own words:

"A large body of evidence renders it certain that human intercourse has, at least, a share in the propagation of the disease, and that it under some circumstances is the most important, if not the sole means of effecting its diffusion. . . .

"The facts, however, by no means sanction the belief that cholera is always propagated in this way. On the contrary, it is certain that the extension of the disease over large towns, if not over larger areas, may take place independently of communication between the sick and the healthy. This is proved by the frequent outbreak of the disease within public establishments, such as prisons and lunatic asylums, in almost every case without a source of infection being traced, and likewise by the rapidity with which the arrival of an infected ship, or the occurrence of the first indigenous case in a large city, is followed by the appearance of the disease in various and distant parts of the city; the extension of the epidemic having, in some of these cases, been so rapid that several hundreds, or even thousands, of persons have perished in the course of between two and three weeks.

"In the cases where human intercourse cannot have been the means of diffusing cholera, the agent most likely to have conveyed the poison from one spot to another is the wind. The poison of cholera being so dependent on the states of air for its existence, increase, and power of action, and having the capability of passing from place to place, must, it would seem, not only be exposed to the air, but, even though it be in part attached to the surfaces of bodies, must, in part, also float in the air. The statistical facts, showing the extension of the disease from centres, and the successive attacks of different localities, though they might be owing to the transmission of the cause of the disease by human intercourse, are quite in accordance with the view that the poison is scattered by varying atmospheric currents from the foci in which it had been developed and increased; while the extension of the disease through a large city with the rapidity above mentioned is, indeed, explicable by no other agency, if the cause of the disease be a substance of the nature supposed. There are, however, few direct observations which tend to confirm this view, by showing a correspondence between the position of a place newly attacked with regard to an existing focus of the disease and the direction of the wind at the time or just previously; and of the few observations which exist the majority are unsatisfactory. The belief, then, in the influence of the wind rests wholly on negative evidence and inference.

"If, however, the atmospheric currents, as is most probable, share with human intercourse the office of disseminating cholera, their part would seem, from the facts communicated to the Cholera Committee, as well as from theoretical considerations, to be rather the diffusion of the disease over limited areas—its transmission from some spots to others near at hand—than its conveyance to distant

places, which is probably effected, in a majority of cases, by the locomotion of men. But the proportion of instances in which the introduction of the epidemic into towns, parts of towns, and individual houses or public establishments, has been due to the one or the other mode of diffusion, cannot at present be determined.

"The propagation of the disease by human intercourse does not prove its contagious nature. If the poison of cholera increases in, or under the influence of, damp and impure air, and is likewise capable of attaching itself to the surfaces of bodies, to the walls of rooms and to furniture, it will also be collected by the clothes of persons living in infected dwellings, will be carried by them from place to place, and, wherever it meets with the conditions favourable to its increase and action, will produce fresh outbreaks of the epidemic. That its propagation in such a mode as this is at least more frequent than its communication by virtue of true contagion is to be inferred, from the impossibility of tracing communication between the first and subsequent cases at the commencement of the epidemic in a large city; from the apparent impossibility that any direct communication can have taken place in many of these cases; still more decidedly from the great rapidity with which the disease sometimes spreads at once through the whole population of a city; from the influence of season and temperature, and of the characters of localities on the rate of the diffusion of the epidemic; and from the occasional alternations of its intensity during its prevalence in a town. The ultimate cessation of the epidemic throughout a country, and even a continent, the restriction of its course in crossing a continent to a tract of comparatively limited extent, furnish, perhaps, still stronger objections to the theory of contagion; for not only are they, like most of those before mentioned, characters which diseases known to be in the strict sense contagious do not present, but they suggest the belief that the propagation of the disease cannot be maintained by any matter emanating from the bodies of the sick.

"Some facts, which constitute presumptive arguments, of more or less force, in favour of the dependence of the epidemic on contagion, namely, the relation, as a general rule, borne by the numbers of the population of a town, and even of a public establishment, to the duration of the epidemic there, the successive attack of the different inmates of a house, or of the ward of a lunatic asylum, the ultimate cessation of the disease after a limited number of days in each house or ward, and the fact that, in the cases where the introduction of the disease into a locality has been traced to human intercourse, the supposed vehicle of the infection has usually been a person already suffering from the disease, or clothes or bedding which had been used by the sick in other places,—all these facts have been found susceptible of explanation in other ways; though the explanations offered have in some instances been necessarily of a conjectural nature.

"With reference to two other arguments, which, if established, would only prove that cholera is in some cases contagious, the evidence examined has been found contradictory. The frequent communication of the disease by the clothes or bedding of the sick to the persons who handle or wash them, under circumstances rendering other sources of infection than emanations received from the bodies of the sick improbable, appears to be by no means proved. The preponderance of evidence is, in fact, opposed to its occurrence. On the other hand, the evidence respecting the especial liability of nurses and others attending on the sick to suffer from cholera, though conflicting, is, in some instances, of such a character as to preclude the absolute rejection of the view that the disease has a contagious property, even though it does not usually spread by virtue of contagion." (pp. 218—22.)

In all probability, Dr. Baly thinks, the disease is *not* spread by the agency of the drinking water, as surmised by Dr. Snow. We may mention here, that in analysing Dr. Muller's 'Report on Cholera in Russia,' in our number for January, 1840, we noticed the statement that

the poison of cholera was said to penetrate the uppermost stratum of water, and to be thus carried along water-courses. We have not heard of any additional evidence on this point.

With respect to the relative frequency with which cholera is transmitted from place to place by human intercourse, or by the agency of currents of air, no exact expression of opinion is made by Dr. Baly; but it is evident that he attributes by far the greatest importance to the former agency. In fact, the passage of cholera across a sea from one continent to another, is held always to occur in this way; and the influence of currents of air is thought to be limited, and to operate chiefly in areas of small extent.

Dr. Baly does not deny that the material cause may be reproduced in the body, i.e., may be truly contagious; but he thinks that "true contagion bears a small part in the propagation of the epidemic," and leaves the question open to further inquiry.

We should not thus content ourselves with stating Dr. Baly's general conclusions, did we not believe that all will study for themselves the minute and valuable evidence on which they are based. And as we believe the work will be universally read, it would be a waste of space on our part, and an injury to Dr. Baly's clear and condensed style, to give an abstract of his immense array of facts.

Leaving, then, the details, we shall now venture to criticise one or two points which appear to us to be less firmly based than the rest of the conclusions.

We have no doubt that the numerous examples and arguments brought forward by Dr. Baly of the conveyance of cholera over sea in ships, will appear to most of us sufficient to prove that this cause is operative to a great extent. But we question whether it is so frequent a cause as Dr. Baly supposes it to be.

Allusion is made to a well-known statement, that the cholera has been known to travel against the wind, and in the very teeth of the Indian monsoon.

"In India the disease has travelled for some hundreds of miles, and during several months in the teeth of the monsoon, and is, therefore, at least in some cases, independent of the influence of the wind." (p. 218.)

As Dr. Baly only admits those two modes of transmission—currents of air and human intercourse—it follows that, if cholera travels *against* the wind, its transmission must be referred to human intercourse.

After attentively reading the volume, the only examples we can find of cholera travelling against the wind are thus given:

"The course of the disease along the main roads of a country is a fact which demands careful examination. Well-marked instances of it are undoubtedly rare, but one has already been referred to, namely, the passage of the disease along the great road leading from Nagpoor to Jalnah, and thence to Aurungabad and Bombay. And it is with reference to this example of the transit of cholera in the line of roads that one of the arguments in favour of its dependence on human intercourse has principally been urged.

"The general direction of the roads leading from Nagpoor to Bombay is from the north-east to the south-west; and cholera extended along that line between the months of May and August, 1818. At the same period, namely, between the 15th of May and the 8th of October, the disease travelled along the east

coast of the peninsula from Chicacole and Vizagatapam to Madras, in nearly the same general direction. Now, the south-west monsoon prevails from the middle or the end of May till October. In both these instances, therefore, the disease seems to have travelled 'in the teeth of the monsoon.' The direction of the wind during the prevalence of the monsoon is modified, from time to time, in the interior of the peninsula, and also on the east coast, by the influence of high lands and by thunder-storms, generally attended by north-west winds, but it does not appear to be reversed so completely or so frequently, even in limited spaces, as to countenance the belief that the progressive passage of the disease from the south-east towards the south-west, over several hundred miles during the summer season in India could be due to the agency of currents of air passing in that direction.

"If, then, the extension of the disease in these instances was not effected by atmospheric currents, to what agency was it due? The numerous well-established facts, tending so strongly to show that the cause of cholera is a morbific poison, the increase of which is dependent on certain conditions of the atmosphere, leave two hypotheses as the only alternatives; one that the poison was carried by men, or through their influence, and the other that it diffused itself, in the manner of the admixture of gases, through the atmosphere more rapidly than the air itself moved. The latter hypothesis is, of course, opposed to all those facts already examined, which afforded apparently such firm grounds for the belief that the poison was not diffusible in its nature. On the other hand, the hypothesis of the conveyance of the poison by human means is not inconsistent with the history of the epidemic in England, and as applied in these instances to explain the march of the disease in opposition to the atmospheric currents, certainly finds support in facts which must now be noticed."

When, however, those cases are closely examined, we find some uncertainty about them. It is mentioned (as we have already pointed out*), that on one of the occasions cited above, the cholera took no less than 5 months (3 months, according to Orton) to travel from Masulipatam to Madras, and then passed slowly overland, while between the two places there is a constant passage of native boats, and the voyage occupies only 10 days. As the south-west monsoon is liable to considerable intermissions on the eastern coast of the Indian peninsula, it is quite a reasonable hypothesis, under the circumstances of the case, to suppose that the progress of the cholera was greatly delayed by the monsoon, but that it still travelled when the east and north-east winds occasionally set in. At any rate, it did *not* travel with the shipping, which was the route of greatest intercourse.

When, thus passing with extreme slowness against the wind, it arrived at Madras, the period of the north-east monsoon had commenced, and now the disease acquired a great velocity, and spread very rapidly over the country to the south, although communication was very much interrupted by the monsoon.

In fact, this case seems to us as perfect an example as could be wished of the retardation of cholera by an adverse, and its rapid transmission by a favourable, wind.

In the second case cited by Dr. Baly, we find the facts somewhat similar. The disease certainly passed from Nagpoor to Jalnah while the south-west monsoon was blowing, but it took no less than 6 weeks to travel these few miles, while between the two places travellers were con-

* See British and Foreign Medical Review, April, 1847, p. 382.

stantly passing. It was, however, declared to have been at length brought by a detachment of troops, and the case is one of the most celebrated and most discussed instances of contagion (portability, we should rather call it) of the first epidemic.*

If these are the only two instances which can be found of the passage of cholera against the wind, we must consider them as of little value. Dr. Baly, who has evidently very carefully perused the whole history of the subject, cites no other instance, and we are not able at present to recall to mind any less equivocal example. We have heard that in Russia, in 1848, it seemed to pass against the wind, but we have no definite information on the point.

A good example of the influence of the wind is given by Jameson. It is that, on investigating the reason why cholera had so singular a tendency to travel to the north-west during the first epidemic in Bengal, it was noticed that the wind, in the great majority of cases, was blowing from the opposite quarter.

We are inclined to think, then, that Dr. Baly has underrated the influence of the wind; and, while we fully admit that it may very likely be the case that, in a long transit with currents of air, the material cause may perish by decomposition, we must remember that there are scarcely any facts which can show us whether this is the case or not.

A statement connected with the transmission of cholera overland has been frequently made—viz., that it passes along the great trunks of communication, and never faster than a man can travel.

We are disposed to cavil at both these statements, as thus shortly expressed, since, as mentioned by Dr. Baly, if cholera spreads along the course of rivers, other circumstances come into play, as moisture, effluvia, &c.; and when it spreads along roads (of which, however, there are few examples), it may merely find, in the more crowded country thus presented, its conditions of development in greater abundance.

As to its rate of travelling, it must be remembered that, if it has seldom travelled *faster* than a man, it has generally travelled very *much more slowly*.

We have already referred to the case of Masulipatam and Madras. We may refer to one still more remarkable, which occurred in the first Indian epidemic, and which we have already quoted in the article formerly mentioned. It is the case of the 69th regiment, attacked in 1818 near Seringapatam, while on the march to Cannanore. The regiment carried the disease with them for some time, but finally outmarched it, arrived at Cannanore healthy, were put into quarantine, and afterwards entered the town. Cholera, however, followed them on the same route, and attacked Cannanore a sufficient time after the entrance of the regiment to do away with all suspicion of importation by its means. In Russia, in 1848, the cholera traversed 675 miles in two months, which (if we take two months to represent 60 days) would give only 11¼ miles per diem. It may be said, that it is of no consequence to the argument whether cholera travels exactly at the same rate as a man travels, or more slowly. This is true; but we doubt whether the inference which is intended to be drawn from the expression above quoted would be so easily made if it is explained that the rates of travelling of men and of cholera are not identical.

Admitting that the transmission of cholera is to be explained by the conveyance of its cause by men, or by being driven by the wind, and that the respective frequency of these two modes, and of the transmission by contagion, remains to be determined, have we so far exhausted the matter? Is cholera transmitted in no other way? Is not there some other cause, more inexplicable than these, which must be held to account for phenomena which neither ships, nor travellers, nor currents of air, can account for? Is there not, in one word, some unknown force which is connected with the diffusion of cholera, as well as with its development and decline?

It must be conceded that the notion that cholera travelled invariably from east to west is erroneous; its course is too erratic, too much influenced by modifying circumstances, thus to be expressed in a single phrase. Yet still we are not prepared to agree with those who reject altogether this common opinion. On two several occasions, cholera has reached England by the same route from India, and this route has not been the one of greatest intercourse. Although it has not invariably tended to the west and north, yet, with many deviations, this has been its general direction,* and in this there appears to be a considerable analogy with the path which influenza has frequently, though, also, not invariably followed. Then, when epidemic in a place, after making allowance for all the meteorological changes, and other circumstances, we have seen that Dr. Baly has been led like others, to conclude that there is yet some peculiar and unexplained condition, which has as great influence on its increase and its decline. We are not fond of mysterious and occult causes, but we must ask, is there not, in the remarkable travelling power of cholera during certain years, and then only, in the peculiarity of its increase, in its as singular disappearance, an indication of a yet unfathomed force, which, influenced as it evidently and in a great degree is, by geographical, meteorological, and sanitary conditions, yet stands in most intimate relation to the development of the disease?

Among the most interesting facts communicated by Dr. Baly are some which appear clearly to show the presence of an unusual condition or force in England, in 1848-9. Thus, it is pointed out how the extraordinary mortality of diarrhœa in 1849, in almost all parts of England, indicated the presence of some fresh morbific agent; again, it is shown that the early part of the first epidemic had, in most of the places attacked, a faintly-marked climax in January, 1849; while the registrar-general has found that the second climax, in the autumn of 1849, occurred also, nearly at the same time, in the eleven great registration divisions of England.

As we had, doubtless, in 1849, a general condition which invaded us, and then ceased, and which regulated the grand mutations of the epidemic, and as this same influence must have passed over the continental countries which suffered, and had ceased to suffer, before the disease reached England, we really see no alternative but to concede the probability of some motive agency, in addition to winds and human intercourse, which thus impelled cholera over so large a portion of the earth's surface. And when we remember, that in the history of past epidemic diseases, such as the black death, or of diseases which still prevail, as the influenza,

* See M. Lasègue's Account of the Russian Epidemic of 1847: British and Foreign Medico-Chirurgical Review, Jan. 1849, p. 7.

we have also evidence of similar locomotion, the inference drawn from the history of cholera receives, it appears to us, considerable support.

The concluding chapter of Dr. Baly's Report is occupied with a consideration of the means of preventing the spread of cholera. It is characterized by the same careful, judicious, and enlarged views as the rest of the Report, and will, doubtless, be of great aid in any future legislation on this point.

We must now turn to Dr. Gull's "Report on the Pathology and Treatment." This has been a less difficult task, but has been executed with equal skill and success. "It is not intended as an essay on the disease," but is meant to include the principal ascertained facts. The information is drawn from all accessible sources, and is distributed under the following heads:—1. Condition of the body after death. 2. Morbid appearances after death in collapse. 3. Morbid appearances after death in reaction. 4. Pathology. 5. Treatment.

Under the second and third heads a very good account is given of the researches of Reinhardt and Leubuscher, of Virchow, Leudet, Pirogoff, and other continental observers on the morbid anatomy; and the labours of British pathologists are, of course, not neglected. A tabular view is given of the morbid appearances in fifty-eight cases, most of which were supplied to the Committee, in answer to the questions sent out by them.

The important researches on the chemistry of the blood by Garrod, of London, and by Schmidt, of Dorpat,* are given in some detail. Dr. Garrod's able paper must be familiar to all our readers. The observations of Schmidt have an interest apart from the subject of cholera, as it was in this work that correct views of the mode of analyzing the blood were first laid down with sufficient clearness. As it is difficult to overrate the importance of Schmidt's researches, we shall select this portion of Dr. Gull's work for comment, rather than dwell on the familiar details of the morbid anatomy.

Schmidt attempted to trace out the exact chemical steps which attend the period of transudation from the blood into the intestinal canal, and to give, so to speak, a formula of these changes. As far as the inorganic constituents are concerned, this formula may, we presume, be considered to be as precise and comprehensive as the present state of chemistry will allow. The condition of the organic constituents, however, was much less clearly ascertained, on account of the imperfect way in which chemistry as yet deals with these bodies; and although Schmidt devised an entirely new method of investigation, which consisted in observing whether the changes of healthy and of choleraic blood, when brought into contact with various decomposing and fermenting substances (urea, sugar, amygdaline, asparagine), were identical, it led to no precise result, on account of the cessation of the epidemic, and the most important problem in the chemistry of cholera remained unsolved. We need, therefore, scarcely remark, that an inquiry into the causes of the transudation cannot yet be attempted; and it is evident that the investigation of the organic constituents of the blood in cholera surpasses the present powers of organic chemistry.

* Charakteristik der Epid. Cholera. Von Carl Schmidt. Leipzig und Mitau, 1850.

The results of Schmidt's observations, though imperfect, are, however, of extreme interest. He thus defines cholera:

"Separation of the water and of the salts of the intercellular fluid (of the blood) through the intestinal canal. Retention in the blood of an important excess of albumen, and of blood-cells, with apparent loss, but (if the albumen be taken as unity), in reality great diminution of the salts and fibrine." (p. 36.)

We need scarcely observe that this is not really a definition, but only an enumeration of some of the most prominent phenomena of cholera.

In the transudation of cholera (a period of short duration = 36 hours, *Schmidt*), the intercellular fluid of the blood (i. e., serum and fibrine) is first affected, and water, salts, and a small portion of albumen pass off, and form the well-known liquid-stools.

The order in which the constituents of the serum are affected is thus laid down by Schmidt. The water transudes before the solids of the serum; the inorganic before the organic solids; the chlorides before the phosphates; the salts of soda before the salts of potash. The causes of this order of transudation are as little known as the cause of the transudation itself; but it is interesting to observe that the order is very much the same as takes place during the action of some purgative medicines, as elaterium.

Very soon after the transudation of some of the constituents of the serum commences, an important change occurs in the blood; the normal diffusion-currents between the fluids in the blood-cells and around them alter; and the constituents of the blood-cells transude into the serum in the same order as the constituents of the serum transude into the alimentary canal; that is to say, the water diffuses more readily than the solids; the inorganic solids more readily than the organic; the chlorides, and of these the soda salts, more readily than the phosphates. The result is, that at the height of the transudation period, the constitution of the blood is profoundly altered. Of course the per centage of the organic constituents (which pass off with so much difficulty) is enormously augmented; it is probable that their composition must have undergone alteration, but of this nothing is known. The inorganic constituents of the blood, if compared to the water, are at first (during the first four hours) increased, because at this time the water is passing off with great rapidity; afterwards, as the salts pass off, the disproportion is lessened, and after eighteen hours, the proportion of salts is greatly diminished, and if compared with the organic constituents, the diminution is enormous. With respect to the individual salts, there is in the blood a relative preponderance of phosphates over chlorides, and of potash salts over soda salts. This follows, indeed, as a matter of course, if the formula for the order of transudation, already given, be correct.

Schmidt has endeavoured to express the whole process of transudation in exact figures, and it may not be uninteresting to quote his chief results. For this purpose he selected a cholera patient (female) who had been in perfect health up to the moment of attack; a very complete examination was made of the blood after eighteen hours' vomiting and purging, and of all the fluids transuded into the alimentary canal during this period. This blood was then compared with that taken from a healthy female of the same race, age (nearly), habits, and bodily conformation, and it was

assumed that the composition of this blood would represent, with sufficient accuracy, the blood of the cholera patient immediately before the attack. Admitting this, the following figures (in which fractions are neglected) will be found interesting. We will speak first of the blood-cells, and then of the intercellular fluid (serum and fibrine):*

I. *The Blood-cells.*

(*a.*) The blood-cells (of the whole body) of this patient contained (presumably), before the attack of cholera, 87½ ounces (English) of water (in round numbers). During eighteen hours' transudation—i.e., choleraic vomiting and purging—12 ounces diffused from the cells into the intercellular fluid (serum and fibrine).

(*b.*) Before the attack the blood-cells contained (in round numbers) 18,500 grains of organic solids, (globuline, hæmatine, &c.) During eighteen hours' transudation, 382 grains diffused into the intercellular fluid. As comparatively much more water had diffused away, the contents of the blood-cells were left of course much richer in organic constituents.

(*c.*) The blood-cells contained, before the attack, 548 grains of inorganic substance, (various salts, iron, &c.) After eighteen hours' transudation, no less than 182 grains had diffused into the serum, so that the blood-cells were left very poor in salts as well as in water, though so rich in organic materials.

(*d.*) The healthy blood-cells contained 218 grains of potassium (all the potash being calculated as potassium); of this quantity 46 grains diffused into the serum in eighteen hours.

(*e.*) The healthy blood-cells contained 100 grains of sodium; in eighteen hours nearly 62 grains diffused away.

(*f.*) The healthy blood-cells contained 63 grains of phosphoric acid; in eighteen hours only 5 grains diffused away into the serum.

(*g.*) The healthy blood-cells contained 99 grains of chlorine, and lost, in eighteen hours, by diffusion into the serum, 29 grains.

(*h.*) The sulphuric acid in the healthy blood-cells, consisting only of 4 grains, passed entirely into the serum; so, also, did the chalk and magnesia, consisting of about 6 grains.

The blood-corpuscles were thus left, at the end of eighteen hours, in a most abnormal condition; the great loss of water and of salts, and especially of the chloride of potassium—that important constituent of the blood-cells—would at once lead us to conclude that their functions must have been greatly impaired. Schmidt accordingly found that the amount of oxygen contained in them was lessened by almost one half.

II. *The Intercellular Fluid.*—(Serum and Fibrine.)

Before the attack, the intercellular fluid of the whole body was composed as follows:

* For the mode in which Schmidt determines the relative amount of blood-cells and serum we refer to the work, or to Lehmann's Physiological Chemistry, or to Carpenter's Physiology, 4th edition, p. 151, 2.

1. Water	178	ounces.
2. Organic solids (fibrine, albumen, &c.)	7212	grains.
3. Inorganic solids	782	„
4. Potassium	30½	„
5. Sodium	296	„
6. Phosphoric acid	55	„
7. Chlorine	339	„
8. Sulphuric acid	9¼	„
9. Lime and magnesia	23	„

During the period of vomiting and purging, the serum gained gradually, also, so much water, organic solids, salts, &c., from the blood-cells, as is given above.

During eighteen hours' purging, and vomiting there was measured 183 ounces of fluid, derived from the alimentary canal; 70 ounces of this had transuded from the blood, and 113 had been taken as drink. In the transuded 70 ounces were contained—

1. Organic matter	366	grains.
2. Salts	474	„
3. Potassium	30	„
4. Sodium	165	„
5. Phosphoric acid	13	„
6. Chlorine	230	„
7. Sulphuric acid	12½	„
8. Lime and magnesia	12½	„

Some curious data can be made out from these numbers. The composition of the blood-cells was only altered, so to speak, by loss; the composition of the serum was altered both by what was lost in the intestinal canal, and by what was gained from the blood-globules. Thus, 389 grains of organic matter diffused into the serum, but only 366 grains passed from the serum into the intestines; so that the serum, at the end of the transudation, was absolutely richer in these constituents. So, again, the serum, at the end of the vomiting and purging, was richer in potassium, for it had gained 46 grains from the globules, and had lost only 30 by the purging.

On the other hand, the gain from the blood-globules by no means compensated the enormous drain from the serum, of the chlorine and sodium. The gain of phosphoric acid was also insufficient to make up the loss, although this was comparatively much less than in the case of the chlorides.

The utter breaking-up of the normal composition of the blood is thus very evident; but this is not all.

During the transudation into the intestinal canal it would appear that the diffusion-currents *from* the blood into various structures are diminished, while, on account of the density of the blood, the inverse-currents from these structures *to* the blood, are augmented in rapidity. In this way fluids are drawn from the muscles, the organs (except, probably, the brain), and, in fact, most of the tissues; and it is probable that these fluids are charged with substances (such as sugar, inosite, lactic acid, &c.) which, under ordinary conditions, are taken very much more slowly into the blood, and are soon decomposed when they get there. The extent to which the blood is contaminated and injured by this admixture, and

by the retention of urinary constituents, is, however, not yet accurately known.

Some writers appear to think that the rapid withdrawal of water from the muscles and nerves will explain the cramps better than the hypothesis, that they are reflex spasms from irritation of the alimentary canal; but if this were so, the cramps would be in proportion to the collapse, which is not the case.

In the case just related, Schmidt supposes that about four ounces and a half of water were lost by evaporation from the skin and lungs, so that the sweating in this instance must have been slight.

The foregoing statements are to be received as simple matters of fact, but they may be made to yield, or, we should rather say, they almost lead up to, an important inference.

When we remember the great share taken by the blood-globules in the respiratory and heat-furnishing processes, it is scarcely possible to avoid concluding that their loss of salts is connected with the characteristic cyanosis and lowered temperature in cholera. It is a matter of familiar observation, that, in *most* cases, there is vomiting and purging (often called the premonitory diarrhœa) before there is loss of heat, though this very soon follows, in a slight degree, and then gradually augments. In other words, the diarrhœa coincides with the first chemical alteration in the blood, the transudation of some of the constituents of the serum; and the lowered temperature follows afterwards at the time when we know that the diffusion from the blood-cells into the serum must be taking place, and augments gradually as the diffusion increases; it is, therefore, a matter of strong temptation to trace up all the phenomena of the disease from the starting-point of transudation of serum-constituents: if we admit only that, from some unknown cause, the water and the chloride of sodium of the serum transude through the intestinal canal, and continue to do so for a specific number of hours, all the other chemical changes in the blood, and the most marked symptoms, such as the abnormal respiratory symptoms, seem to follow as a matter of course.

As we have already seen, in the definition we have quoted at a former page, Schmidt does not hesitate to adopt this view of the disease, and an early theory of the nature of cholera has thus received the support of one of the best chemists of the day.

We question, however, whether this is not too limited a view, when judged of by the necessary test of clinical observation. It would delay us too long to debate this question fully at present; but Dr. Gull has the following judicious remarks, which seem to us to express very correctly the opinion we ought provisionally to hold:

"As many of the symptoms of the stage of collapse depend upon the loss of fluid, it has been too absolutely inferred that the general phenomena of the disease are always in a necessary relation to the amount of these effusions.

"In tropical regions, where either the intensity of the poison is greater, or the predisposing conditions of constitution are more favourable to its operation, it appears to be by no means unfrequent for the strongest subjects to fall into sudden collapse, without any very notable loss of fluid. Such facts are authenticated by so many careful writers, as to leave us in no doubt of their occurrence. In temperate regions, such cases, though rare, are not unknown, and in a less marked degree are within the experience of most who have seen the disease in its severer

forms. Even when the loss of fluid is very great, it is doubtful whether death is due to it alone, since we often see patients in an apparently equally hopeless state, collapsed, and bloodless, whose tissues, so soon as the nervous system begins to react. recover their elasticity before any amount of absorption could, from the circumstances of the case, have occurred." (p. 131.)

Some evidence is afterwards given on these points, and Dr. Gull then says:

"Notwithstanding the incompleteness of the data, we conclude that, although, in a large number of instances, the intensity of the symptoms is in a general way proportionate to the amount of the effusion, yet that this will only in part explain the attendant collapse which often appears to be in no inconsiderable degree due to the adynamic state of the ganglionic nervous system, induced either primarily by the poison, or secondarily by the lesions of the affected mucous surface. A further elucidation of this subject is yet a desideratum." (p. 134.)

In spite of these elaborate chemical inquiries, from which so much was hoped, we are obliged to confess that the *treatment* of cholera has not been advanced during the late epidemic. Our more precise knowledge of the steps of the transudation period merely points out that, if the diarrhœa (intestinal transudation) can be arrested in time, the other changes in the blood will probably become impossible. The well-known practical rule, that the transudation is at once to be arrested, receives thus scientific support, but no further step of treatment is pointed out. How, in the collapsed stage, are we to reverse the current which is pouring from the blood into the intestines? How are we to get the potash salts into the blood-globules, and the water into both cells and intercellular fluid? How, in fact, are we to replace, in its wonted order, this extraordinary medley of substances which, in the healthy system. are kept in such singular accuracy within their proper boundaries? To these questions, chemistry is either dumb, or returns a despairing answer. The steps in one direction are traced, but the return appears impossible to science, since the causes of the phenomena are totally unknown: we are driven back on our empirical knowledge, and what is termed the scientific treatment of cholera must not yet be looked for. Dr. Gull gives us no less than 57 pages on the subject of treatment, and almost every plan is described and judged of. We shall not pause on this chapter, but beg to recommend its most attentive perusal; it indicates, at any rate, if not some invariably successful treatment, the most efficacious empirical means; and shows us, also, what measures—and there are not few—are dangerous in the treatment of cholera.

We have passed over without comment some very interesting chapters on the so-called "premonitory diarrhœa," and on the influence of age and sex on the mortality. Many of the facts are derived from the registrar-general's report, and have been given in former numbers of this journal; but there are many new facts well cemented in with the old ones, which give a great deal of originality to this part of the work.

In taking leave of these Reports, we can conscientiously say that we do not think the College of Physicians could have made a more fortunate selection than the two gentlemen to whose labours we owe so much valuable instruction. We believe that their work will be, in future years, constantly referred to, and when so referred to, we are confident it will always be with pleasure and profit.

Since these Reports were completed, an important paper has been partly published by Dr. Lauder Lindsay, of Dundee.* It would seem probable that this gentleman has solved the question of the contagion of cholera by communicating the disease to animals. We defer, however, any conclusion on this point, till the whole of the experiments have been made known.

E. A. Parkes.

Review XI.

1. *The Chapter on Respiration, in 'Lehmann's Lehrbuch der Physiologischen Chemie.'* Dritter Band. Zweite Auflage, 1853. pp. 284–346.
2. *The Article 'Respiration,' in the 'Cyclopædia of Anatomy and Physiology.'* By John Reid, M.D. 1848.
3. *The Chapter on Respiration, in the 'Principles of Human Physiology.'* By William B. Carpenter, M.D., F.R.S. Fourth Edition, 1853.

We must premise, at the outset of this article, that we shall confine our attention in the following pages almost exclusively to the Chemistry of Respiration—a term indicative of the process by which animals absorb oxygen from the surrounding atmosphere, and exhale a nearly corresponding quantity of carbonic acid.

Before proceeding to notice the changes which the respiration of the animal world impresses on the circumambient air, it may be expedient to refer briefly to the present state of our knowledge regarding the atmosphere itself.

The chief constituents of the atmosphere are oxygen, nitrogen, carbonic acid, ammonia, and watery vapour. Neglecting fractional parts, the oxygen and the nitrogen stand to one another in the ratio of 21 to 79 by volume, or 23 to 77 by weight; the other substances which we have named occur, as we shall presently see, in extremely minute, often almost imperceptible, quantities: they seem, however, to be always present. To the above list we should probably add nitric acid, ozone, and iodine.

To these principal and apparently essential elements many others must be added, which may be regarded as incidental. They are thus briefly summed up by Mulder:†

> "Volatile exhalations from the organised matter of the soil; products of the volcanoes on the earth's surface, and of the artificial burning of fuel; gaseous fluids escaping from mines; vapours of volatile solid substances; hundreds of volatile oils from odoriferous plants; putrid volatile products of animal and vegetable decomposition; the hydrochloric acid arising from salt water in shallow lakes and lagoons; exhalations of men and animals; an innumerable multitude of substances ascending in vapour from manufactories and chemical processes; and finally, the volatile products of animal excrements; all these are sources of pollution to the atmosphere, which thereby undergoes innumerable alterations in its composition."

We need hardly remark that the accumulation of these noxious elements is prevented by atmospheric currents, and more especially by the rain which, again to quote the words of Mulder,

> "In falling, carries with it everything that floats in the atmosphere, and which is not essential to its constitution; which brings back to the earth what came

* Edinburgh Medical and Surgical Journal, April, 1854.
† Chemistry of Vegetable and Animal Physiology.

from the earth; and which, while it thus purifies the atmosphere from these hurtful adulterations, restores to the soil these numberless volatile substances, where they have abundant opportunities of forming not only harmless, but useful combinations."

We will now consider, with rather more minuteness, the composition of atmospheric air, from which it is supposed that the above-named impurities have been removed by the causes which we have already indicated.

The first point is to determine accurately the ratio in which the oxygen and nitrogen stand to one another. The earliest experiments on this subject are comparatively recent, for the whole science of weighing and measuring is scarcely seventy years old, and the true determination of the composition of the atmosphere obviously cannot date further back. At the close of the last, and the beginning of the present, century, it was believed that there was no constant ratio between the oxygen and the nitrogen; and in proportion to the greater or less amount of oxygen, the air was supposed to be better or worse fitted for respiration. In 1804, Humboldt and Gay Lussac published a memoir, in which they maintained that the ratio between these gases is invariable. In twenty-nine experiments, instituted on twenty-nine different days during the months of November and December, in dry and in rainy weather, and during various winds, with the air of Paris collected above the Seine, their largest quantity of oxygen was 21·2 (by volume), and the smallest 20·9—a difference lying quite within the limits of the ordinary errors of analysis. Results confirmatory of the above-named constant ratio were subsequently obtained by De Saussure, with air collected over the Lake of Geneva: by Gay Lussac, with air obtained in a balloon ascent from a height of 21,430 feet: by Humboldt, with air from the Antisana, a mountain 16,640 feet high: by Configliachi, from the Legnone, a mountain 8130 feet high: and more especially by Dumas and Boussingault, in their elaborate memoir published in 1841.

Moreover, impure air, after the removal of the foreign ingredients, has been found to contain these gases in the same ratio. Specimens of air collected over rice-fields (Configliachi), from a crowded Parisian theatre (Seguin, Gay Lussac, and Humboldt), from the wards of a hospital (Edmund Davy), and from a dormitory in the morning (De Saussure), exhibited no difference in this respect.

Doubts, have, however, been recently entertained regarding the rigid invariability of this ratio. Lewy made seven analyses of air collected in November, 1842,* at Guadaloupe; the maximum per-centage of oxygen by weight was 23·14, the minimum 22·68. The air at Copenhagen gave, from the 17th of November to the 22nd of December, 1841, 23 per cent. (by weight) of oxygen, and at Elsineur (which lies upon the sea-coast) the amount was 23·037, while air collected during a journey between Copenhagen and Havre yielded only 22·6. Subsequent investigations by the same chemist confirm, rather than remove, these doubts. Air which he collected from above the surface of the sea (during a residence in South America), was† found to contain more oxygen and carbonic acid

* Annales de Chim. et de Phys., 1843, tom. viii. p. 425.

† Comptes Rendus, tom. xxxi. p. 725.

during the day than during the night, and this difference was more marked in clear than in cloudy weather. He accounts for this difference by supposing that as the surface of the water becomes warmed by the sun's rays, some of the previously absorbed air is evolved, which is known to be richer in oxygen than atmospheric air.* In his last memoir† he gives the general results of a large number of analyses of air, some of the specimens having been collected over the Atlantic, and others in South America. The most important conclusions at which he arrives are that the atmospheric air in New Granada (the mean result from eleven different spots being taken) is the same as that in Europe. The air in New Granada was found to contain rather more oxygen and carbonic acid during the dry than during the rainy season; the differences are, however, very trifling. Regnault‡ has recently published the results of his analyses of air taken from different parts of the earth. In the year 1847, he proposed that specimens of air should be collected (and enclosed in hermetically sealed vessels) on the 1st and 15th of every month, at as many places as possible. Our space forbids our quoting any of his valuable tables: we merely quote a few of his results. The mean quantity of oxygen (by volume) in the air of and near Paris, was 20·96 per cent.; this was the result of more than one hundred analyses, the extremes being 20·913 and 20·999. While specimens of air obtained from Toulon in May, 1851, yielded 20·85 and 20·87 per cent., others from Algiers in the following month yielded 20·42 and 20·395 per cent. Of eleven specimens collected over the sea near the south coast of Asia, two presented an abnormal quantity of oxygen; air from the Bay of Bengal contained 20·46 and 20·45; and air from over the mouth of the Ganges (in March, 1849, when cholera was breaking out) 20·39 per cent. The air obtained by Sir James Ross from the Arctic Regions contained the normal quantity of oxygen. In conclusion, Regnault expresses his belief that the atmosphere presents appreciable, although extremely minute, variations in its amount of oxygen: the variations in general fluctuate between 20·9 and 21·0 per cent.: but in certain cases, especially in hot climates, the amount may fall as low as 20·3 per cent.

The amount of carbonic acid is by no means so constant as that of oxygen or of nitrogen. In 1827 and 1829, De Saussure made 225 experiments, with the view of determining the proportion in which this gas exists in the atmosphere. From 104 experiments with the air at Chambeisy, a village near Geneva, it appeared that the average quantity of carbonic acid (by volume) in 10,000 parts was 4·15, the maximum being 5·74, and the minimum 3·15. He ascertained that during the day the air contained less carbonic acid than during the night, the average in the day being 3·38, and in the night 4·32, the respective maxima being 5·4 and 5·74. When gentle breezes prevailed, the proportion of carbonic acid was smaller than when the wind was violent. The proportion was less diminished by violent showers than by gentle continued rain. There

* Morren has likewise ascertained that air collected on clear sunny days, on the surface of sea-water in which algæ abound, contains 28 per cent. (by volume) of oxygen. He attributes this excess to the rapid escape of oxygen from the surface of the water and of the sea-weeds under the influence of the sun's rays. (Ann. de Chim. et de Phys. tom. xii. p. 84.)

† Ann. de Chim. et de Phys., tom. xxxiv. p. 5; or Comptes Rendus, tom. xxxiii. p. 345.

‡ Ann. de Chim. et de Phys., tom. xxxvi. p. 385.

was found, as might be expected, more carbonic acid over cultivated grounds than over the Lake of Geneva, and more in the air of the town than in that of the village of Chambeisy.* Lastly, he found that mountain air contained somewhat more carbonic acid than air at the level of the sea. Verver's† results, at Gröningen, give 4·2 as the average, 5·1 as the maximum, and 3·5 as the minimum, in 10,000 parts of air.

Boussingault, who made a continuous series of experiments for nine months with the air of Paris, found, as a mean, 4 parts, the maximum and minimum being 6·7 and 2·2.

The brothers Schlagintweit have recently experimented on the amount of carbonic acid in the higher strata of the atmosphere. In their first memoir‡ they state, that the air on the eastern Alps contains from 3·2 to 5·8 volumes of this gas in 10,000; there being a progressive increase to the altitude of 3366 metres, or 11,073 feet, where a constant maximum seems to be attained. In the second memoir,§ there is a record of experiments made with the same object in the neighbourhood of Monte Rosa, at elevations varying from 3162 to 4224 metres, or from 10,402 to 13,896 feet, above the level of the sea. In 10,000 parts of air at these altitudes, the mean quantity of carbonic acid was 7·9, and the maximum 9·5. The largest quantity occurred in fine bright weather, and the least (5·9 parts) on a cloudy day. In Berlin (which is 106 feet above the level of the sea) they found (in three experiments) that the carbonic acid ranged from 3·9 to 4·9 parts.

This subject is likewise investigated by Lewy in the memoirs to which we have already referred, and by Menc.||

Considerable interest attaches to the question regarding the presence of ammonia in the atmosphere, in consequence of the important part which, according to Liebig, it plays in the nutrition of vegetables. We must not here enter upon this disputed topic; we will merely observe, that Mulder declares that it is impossible for plants to derive their nitrogen from this source. A subsequent assertion by the same distinguished chemist, that it has not yet been found possible to weigh the quantity of ammonia contained in the atmosphere, has been disproved during the last three or four years by various experimenters, amongst whom we may especially mention Fresenius and Horsford. From observations made during 40 days in August and September, 1848, Fresenius¶ calculates that 1,000,000 parts of air contain during the day 0·098 and during the night 0·169 parts (by weight) of ammonia. Horsford** made frequent determinations of the ammonia in the atmosphere from July to December; his results are very different from those of Fresenius; for in 13 experiments, he found that the quantity of ammonia in 1,000,000 parts of air ranged from 47·6 to 1·2; the quantity was greatest in July, and least in December. The subject has been independently investigated

* In confirmation of this result, we may mention that a series of sixteen experiments, made on the same successive days, in September and October, 1843, and at the same time, by Boussingault in Paris, and Lewy, near Montmorenci, gave for Paris, 3·19, and for the country, 2·984 parts of carbonic acid in 10,000 parts of air.

† Quoted in Ann. de Chim. et de Phys., tom. x. p. 462.

‡ Pogg. Ann., tom. lxxvi. p. 442.

§ Ibid., tom. lxxxvii. p. 293.

|| Comptes Rendus, tom. xxxiii. pp. 189 and 222.

¶ Ann. der Chem. und Pharm., vol. lxxii. p. 218.

** Ibid., vol. lxxiv. p. 243.

during the year 1852 by Pierre* and by Ville.† There is, however, no close accordance between the results of any of these chemists.‡

The quantity of aqueous vapour in the atmosphere must obviously differ considerably at different times and in different places, being dependent on various local and incidental causes, as, for instance, the temperature of the evaporating water, the temperature of the air, the velocity of the wind, &c. The most accurate experiments on this subject with which we are acquainted are those of Verver, instituted in the Netherlands. In 1000 volumes of air, the maximum was 10·18, and the minimum 5·8. The average of 50 observations during May, August, and September, was 8·47. From an early hour in the morning to 10 A.M., it was 7·97; from 10 to 2 P.M., 8·58; and from 2 till the evening, 8·85.

We now proceed to the consideration of those substances whose invariable presence is less well established. Of these, the first on our list is nitric acid. Heller§ announced in 1851 that this substance is always present in the atmosphere, basing his opinion on the fact, that paper saturated with carbonate of potash or of soda, after prolonged exposure to the atmosphere, contained an alkaline nitrate. A commission, appointed by the Vienna Academy of Medicine, failed in confirming Heller's statements. The fact may, however, be as he represented it, without his inference being correct; for Schönbein has shown that alkaline carbonates after long exposure to the atmosphere, are, in part, converted into nitrates, in consequence of the ozone of the atmosphere oxidizing the nitrogen under certain conditions (as, for instance, in the presence of strong bases), and converting it into nitric acid. The presence of ozone in the atmosphere may be readily detected by test-paper, prepared with a mixture of iodide of potassium and starch. This mysterious substance, by its strong oxidizing power, liberates the iodine, and allows it to combine with the starch. As a considerable number of the members of the Provincial Medical Association are now regularly tabulating their meteorological observations, we trust that we shall soon be able to see whether an excess of ozone gives rise to the various bad effects that have been attributed to it by Schönbein and others.||

It seems certain, from the researches of Chatin and others, that iodine in very minute quantity is present in atmospheric air. Chatin has arrived at the interesting result, that expired air contains only one-fifth of the iodine contained in inspired air.

In Mulder's 'Chemistry of Vegetable and Animal Physiology,' there is a chapter on "The Atmosphere in its Connexion with Organic Nature," from which we have drawn some of the facts recorded in the preceding pages. It concludes with the consideration of the question—Has the atmosphere, since the appearance of man on the earth, preserved the same composition as it previously had? and will it at all times retain the same composition, assuming, of course, that the numbers of mankind are increasing and those of forests diminishing? We have space for little

* Comptes Rendus, tom. xxxiv. p. 878. † Ibid., tom. xxxv. p. 464.

‡ Mene found ammonia in hailstones: from 800 grammes he obtained 2·78 grammes of chloride of ammonium. (Comptes Rendus, tom. xxxiii. p. 918.)

§ Zeitsch. der Gesellsch. der Aerzte zu Wien, 1851.

|| Gräger, who experimented (at Mühlhausen) thrice daily, from December, 1850, to June, 1851, found an excess of ozone during the night. The influence of winds was not very clear, but there seemed to be a maximum with a south-west wind.

more than his conclusions, and must refer to the work itself for the arguments on which they are based.

"If," he observes, "the very first human beings had been able to make eudiometrical observations, it is probable, that in a given bulk of atmospheric air they would have found a little more oxygen, and somewhat less carbonic acid than exists in the present day, provided their method of investigation had been sufficiently accurate."

As plants must restore what man and animals have taken from the atmosphere, when the equilibrium between the two kingdoms of organic nature is destroyed—that is to say, when plants diminish and mankind increase—there will at length be no longer a sufficient quantity of carbonic acid decomposed to preserve what we consider to be the invariable composition of the atmosphere. When this time will arrive we know not; but this we know, that every being which now has life depends for its existence on the presence of about twenty-one parts of oxygen to seventy-nine of nitrogen in the atmosphere; and that whenever this proportion is materially altered, the present creation must cease, to be replaced, in all probability, by other and higher forms of life.

We now proceed to notice the nature of the changes impressed upon the atmosphere by the presence of the animal kingdom.

The breathing organs present an essential difference from the other organs of excretion in this respect, that while excreting one gaseous fluid, they are at the same time absorbing another. In fact, an interchange of gases takes place within these organs, and it is to this process—whether it be effected by lungs, gills, tracheæ, &c.—that we apply the general term *Respiration.*

From a general review of the labours of those physiological chemists who have especially investigated this subject, we may consider it as an established fact, that the blood in the lungs takes up oxygen gas from the inspired air, and yields to it carbonic acid and aqueous vapour in exchange; and further, that it more commonly yields than absorbs a trifling amount of nitrogen.

The first point to be noticed is the relation subsisting between the exhaled carbonic acid and the oxygen that has disappeared in the lungs from the inspired air. It is well known that the volume of carbonic acid is equal to the volume of the oxygen contained in it; hence, if in the expired air there was found a volume of carbonic acid equal to that of the oxygen which had disappeared from the inspired air, it might be inferred that the oxygen absorbed in the pulmonary vesicles was precisely sufficient to form the carbonic acid that was exhaled. This inference was actually drawn by many of the earlier investigators of this subject; it does not, however, stand the test of accurate experimental inquiry, for it is found that, generally speaking, the volume of oxygen which is absorbed is far greater than that of the carbonic acid which is given off; and hence we must conclude, that the former gas serves not only for the oxidation of carbon, but also of hydrogen in the animal organism. If, for instance, we enclose animals in a suitable apparatus, and analyse the air which they have altered by their respiration, we find more free oxygen missing than can have been applied to the formation of carbonic acid; and we arrive at a similar result on instituting a comparison (as was done by Marchand) between the loss of weight of the animal during the

period of the experiment, and the oxygen contained in the expired air, in combination with carbon and hydrogen; when we find that the loss of weight which the animal has experienced is less than would have been expected from the quantities of carbon and hydrogen that have been given off. Hence the body of the animal has gained an appreciable weight during the experiment; and this weight can be due only to oxygen, for the quantities of nitrogen that are absorbed or exhaled are too trifling to produce any perceptible effect. Without at present entering into any detailed notice of the various series of experiments which have been made in reference to this subject, and to which we shall presently refer, we may incidentally remark, that on an average for every one volume of absorbed oxygen only about 0·8516 of a volume of carbonic acid is found in the expired air.

Many attempts have been made to institute a comparison between the volumes of the inspired and expired air. On examining these airs in their dry state—that is to say, after the removal of their aqueous vapour, there must necessarily be, in the latter case, a diminution of volume corresponding to the volume of oxygen that is absorbed, and not converted into carbonic acid. If, however, we compare these airs in their moist state, this rule no longer holds good, for the expired air being usually saturated with vapour, while the inspired air is comparatively dry, it necessarily follows that the tension of the aqueous vapour taken up in the lungs must augment the collective volume of the expired air. We need hardly observe, that the increased temperature of the expired air likewise induces a certain augmentation of volume.

The quantity of water exhaled by an adult male in twenty-four hours has been estimated by Valentin at 506, by Vierordt at 360, and by Horn at 350 grammes (the gramme being equal to about 15·5 grains); but, in all probability, 29 or 30 grammes of this water were contained in the inspired air.

It was long a doubtful question whether, in the process of respiration, there was an exhalation or an absorption of nitrogen; it has, however, been established, both by the experiments of Brunner and Valentin, and those of Regnault and Reiset, that there unquestionably is a slight excretion of nitrogen; the former physicists maintaining that there is an excess of 0·402 per cent. (by volume) of nitrogen in the expired air, and the latter finding, in their experiments on animals, that for every 10,000 parts (by weight) of oxygen that are absorbed, there are from 8 to 133 parts of nitrogen developed in the lungs. Boussingault* and Barral† have independently shown (the former by experiments on animals, and the latter on men) that the amount of the exhaled nitrogen is about one-hundredth that of the exhaled carbonic acid. A portion of the nitrogen of the expired air occurs in the form of ammonia.

Minute quantities of volatile matter, which have been taken either as medicines or with the food, are often to be detected in the exhaled air—as for instance, alcohol, phosphorus, camphor, and ethereal oils; and even when no such substances have been directly introduced into the system, traces of organic matter, composed of carbon and hydrogen, may generally

* Ann. de Chim. et de Phys., 3 Sér., tom. xi. p. 433, and tom. xii. p. 153.
† Comptes Rendus, tom. xxvii. p. 361.

be detected by the reddening which sulphuric acid undergoes when used to dry exhaled air.

Different investigations have led to somewhat discordant results in regard to the quantity of carbonic acid exhaled by an adult man in a given time—as, for instance, twenty-four hours. The origin of these discrepancies might, we suspect, be detected if we could afford space to examine critically the different modes of experiment. According to Scharling, a very trustworthy observer, a muscular man exhales in twenty-four hours, 867 grammes (or 443,409* centimetres, the temperature being 32° Fahr., and the barometer standing at 29·8 inches).

Vierordt calculates that the oxygen absorbed by an adult man in twenty-four hours (part of which is given off as carbonic acid and water, and part retained in the body) amounts to 746 grammes, or 520,601† centimetres; consequently, about 116 grammes of the absorbed oxygen are retained in the organism. According to Boussingault's determinations, about 8 grammes of nitrogen are given off in the same period; and the researches of Valentin show that there are also evolved about 500 grammes of water.

The quantity of carbonic acid in 100 parts of the expired air, estimated by volume (the breathing being perfectly quiet), has been determined by Vierordt, from a very large number of experiments made on healthy men, at 4·334. We extract the following table, bearing on this point, from Dr. Reid's admirable article:

	Average.	Max.	Min.	Extreme difference.
Prout‡	3·45	4·10	3·30	0·80
Coathupe§	4·02	7·98	1·91	6·07
Brunner and Valentin‖	4·380	5·495	3·299	2·196
Vierordt¶	4·334	6·220	3·358	2·86
Thomson**	4·16	7·16	1·71	5·45

As we shall immediately show, the variations in the quantity of the exhaled carbonic acid are dependent on certain conditions of the body and of the surrounding media.

In noticing these conditions, we shall follow Lehmann's arrangement; premising, however, that this portion of the chapter on respiration is little more than a complete abstract of Vierordt's treatise,†† with interspersed remarks and criticisms.

Vierordt commenced his inquiries by attempting to ascertain the con-

* Or about 27,066 cubic inches, if we consider one cubic centimetre to be equal to ·06102523 of an English cubic inch.

† About 31,740 cubic inches.

‡ In some subsequent experiments by Prout, the range in the quantity of carbonic acid was between 2·8 and 4·7; the minimum number occurring once only, and when he was sleepy. His experiments were performed upon himself, and at every hour of the day and night.

§ These experiments were 124 in number, and performed upon himself at almost every hour of the day between 8 A.M. and midnight.

‖ These experiments were 34 in number, and were performed upon 3 males between 33 and 53 years of age.

¶ Vierordt's experiments were nearly 600 in number: they were performed upon himself, chiefly between the hours of 9 A.M. and 7 P.M., and were continued over an interval of nearly 15 months.

** These experiments were made on 10 males and 2 females, and between 11 and 12 in the forenoon.

†† Physiologie des Athmens.

nexion between the amount of carbonic acid in the exhaled air, and the frequency of the respiratory movements.

The following are the mean, maxima, and minima values determined for one minute, when the body was in a state of perfect repose:

	Mean.		Min.		Max.	
Number of the pulse	75·52	...	54	...	101	
" respirations	11·9	...	9	...	15	
Volume of the expired air	384·7	...	256·6	...	568·2	cubic inches.
" " " carbonic acid	16·0	...	10·8	...	27·6	"
Volume of one expiration	31·0	...	22·4	...	42·7	"
Per-centage of carbonic acid in expired air	4·334	...	3·358.		6·220	

These standard values being obtained, he next found that when the number of respirations was doubled (without any diminution of their normal depth), the relative quantity of carbonic acid was 0·907 per cent. less than during quiet normal breathing; when trebled, 1·125 per cent. less; when quadrupled, 1·292 per cent. less; and finally, when increased eightfold, 1·600 per cent. less.

When the respirations were diminished to half their original number—that is to say, 6 in place of 12—the difference in the amount of carbonic acid was 1·316 per cent.

Lehmann arranges these values in the following tabular form:

Respirations in one minute.	Carbonic acid in 100 volumes of expired air.
6	5·528
12	4·262
24	3·355
48	2·948
96	2·662

Vierordt has deduced a formula which (as he believes) expresses the relation between the per-centage of the carbonic acid and the number of respirations in a minute. As, however, the strict accuracy of this formula has been questioned by Marchand and Stürmer,* we pass on to the consideration of the influence which the extent or depth of the respiratory act exerts on the per-centage of the carbonic acid in the expired air. Vierordt has ascertained that—

If the air, after a normal inspiration, contains			4·60	per cent. of carbonic acid,
Then air after one twice as deep		"	4·00	"
"	thrice "	"	3·70	"
"	four times "	"	3·38	"
"	eight times "	"	2·27	"
"	half "	"	5·38	"

Hence, by modifying the frequency and depth of the inspirations, we can throw off a greater or lesser quantity of carbonic acid.

The older experiments of Allen and Pepys, as well as those of Jurine, tended to show that the respired air received most of its carbonic acid in the finest ramifications of the air-passages. Vierordt attempted to decide this point in the two following ways:

* Observationes de Acidi Carbonici Respiratione exhalati Quantitate. Halis, 1848.

He divided each expiration into two as nearly as possible equal halves, the second half being obviously associated with the deeper portion of the lung. In the air yielded by the terminal portions of the expirations, he found, as a mean of twenty-one experiments, 5·44 per cent. of carbonic acid; while, in the corresponding commencing portions, he found only 3·72 per cent.

His other method was to compare the amount of carbonic acid in an ordinary expiration, with that after a deep and laboured respiratory effort. He found, as a mean of eight experiments, that when the carbonic acid of a normal expiration (of 574 cubic centimetres, or 35 cubic inches), amounted to 4·63 per cent., a very complete expiration (of 1800 cubic centimetres, or 110 cubic inches) yielded 5·18 per cent.

From four independent series of experiments, the same observer was led to the conclusion, that any impediment to free respiration produces a very marked diminution of the *absolute*, but a decided augmentation of the *relative* quantity of carbonic acid—a result fully confirmed by the still more recent experiments of Horn.*

We shall now briefly notice the results of the most trustworthy inquiries regarding the respiration of artificial atmospheres of various kinds. Most of the researches bearing upon this point have of course been made upon animals.

It appears, from the experiments of Regnault and Reiset, on dogs and rabbits, that an atmosphere two or three times richer than our own in oxygen produces no very striking results; the animals seemed to suffer no inconvenience, and the products of respiration were the same as in the ordinary atmosphere. This is a result we should have hardly expected, because the earlier observations which had been made by Lavoisier and Seguin, and by Allen and Pepys, on men, and by Marchand on frogs, seemed distinctly to prove, that when *pure* oxygen was breathed, although there was little or no augmentation of carbonic acid, far more oxygen was absorbed than in the normal condition of things: according to Allen and Pepys there was also, under these circumstances, a considerable exhalation of nitrogen.

Numerous experiments have been at different times made on the respiration of air containing an excess of carbonic acid, and on the repeated inhalations of the same air. Marchand found that when frogs were kept in a confined atmosphere they developed less carbonic acid, and absorbed less oxygen, towards the end than at the commencement of the experiment; and that at length they scarcely took up more oxygen than was necessary for the formation of the carbonic acid. Lehmann thinks it probable that an excess of nitrogen is exhaled in an atmosphere rich in carbonic acid. We all know, from Davy's experiments, that pure carbonic acid cannot be inhaled, in consequence of its exciting spasmodic closure of the glottis; and an atmosphere containing about 50 per cent. of carbonic acid is irrespirable from the same cause; air, however, containing a smaller quantity of this gas can be breathed without imminent danger for some time; its unfitness for respiration not depending so much upon the presence of an excess of carbonic acid as upon the absence of a sufficient quantity of oxygen.

* Neue Med.-Chirurg. Zeitung. 1849, pp. 33—39.

Experiments on the inhalation of nitrogen and highly nitrogenous atmospheres have been made by some of the earlier French chemists (Nysten,* Coutenceau,† and Legallois‡), but did not lead to any definite results.

The admirable experiments of Davy on the respiration of nitrous oxide have been subsequently repeated and confirmed by P. Zimmermann.§ It appears, from Davy's analyses of the expired air, that a large quantity of nitrous oxide is absorbed by the blood, and that comparatively large quantities of carbonic acid and nitrogen are given off. Zimmermann, who has experimented somewhat largely with this gas on pigeons and rabbits, found that the pulse very soon became accelerated and irregular, and that the respiration was much quickened; slight convulsions afterwards supervened, and finally asphyxia. A strong rabbit, after remaining for three hours and twenty minutes in an atmosphere of nitrous oxide, was restored by the inflation of fresh air. The same observer found that a rabbit, which, in one hour, produced, on an average, 0·8 of a gramme (about 12½ grains) of carbonic acid in atmospheric air, exhaled 1·3 grammes (about 20 grains), in a similar time, in nitrous oxide.

An atmosphere containing more or less hydrogen may be breathed for a considerable time without injury, provided a sufficient quantity of oxygen be present. Regnault and Reiset placed rabbits, a dog, and frogs, in an atmosphere in which the greater part of the nitrogen had been replaced by hydrogen (55 to 77 per cent. of hydrogen, 1·1 to 14·4 per cent. of nitrogen, and 21·8 to 28·8 per cent. of oxygen); the rabbits inspired this mixture for twenty, and the dog for ten hours, apparently without any injury. At the end of the experiment there was scarcely any loss of hydrogen; there was a greater absorption of oxygen than would have occurred with ordinary atmospheric air; and a little nitrogen appeared to have been exhaled, but this was questionable. These results are almost entirely in accordance with those which had been previously made, both by Lavoisier and Seguin, and by Davy. It follows, from the experiments of Regnault and Reiset, that there is nothing *positively* injurious in the inhalation of pure hydrogen, the reason why such an atmosphere would not support life being referable to the absence of oxygen. Marchand found that frogs, confined in pure hydrogen, died in from half an hour to an hour. When breathing this gas they exhaled far more carbonic acid than in the ordinary air; the hourly quantities for every 1000 grammes' weight of frogs being 0·077 of a gramme in the latter, and 0·263 of a gramme in the former case.

Carbonic oxide, when mixed, even in very small quantities, with atmospheric air, induces a sensation of tightness, suffocation, insensibility, and death. It appears to be definitely established, by the researches of Leblanc‖ and others, that it is to this gas that charcoal vapours chiefly owe their deadly power.

We need hardly remind our readers, that sulphuretted hydrogen,

* Recherches de Physiolog. et de Chim. Patholog. Paris, 1811.

† Revision des Nouv. Doctr. Chem. Physiolog., &c. Paris, 1814.

‡ Exp. sur le Principe de la Vie. Paris, 1812.

§ Dissert. Inaug. Med. Marburg, 1844.

‖ Comptes Rendus, vol. xxx. p. 488.

seleniuretted hydrogen, phosphuretted hydrogen, arseniuretted hydrogen, ammoniacal gas, sulphurous acid, chlorine, &c., are not merely irrespirable, but poisonous gases, like carbonic oxide.

Amongst the various external conditions which modify the respiration, we will first notice the temperature of the atmosphere. The earliest experiments on this subject were instituted on animals which, at a low temperature, fall into a condition more or less closely allied to hybernation. Spallanzani, Saissy, Treviranus, and others, having observed that insects and mollusca, as well as marmots, bats, and hedgehogs, exhaled less carbonic acid at a low than at a high temperature, it was inferred by physiologists that a cold atmosphere produced the same effect on all classes of animals. More recent observations have, however, fully demonstrated, that in the higher animals the exhalation of carbonic acid diminishes with the elevation of the temperature from the freezing point. It appears, from the experiments of Letellier* on greenfinches, pigeons, mice, and guinea-pigs, that these animals exhale most carbonic acid between 23° and 37·4° Fahr., and least between 82° and 110° Fahr. This was even more marked in birds than in mammals; none of the animals could bear a higher temperature than 110°. Marchand has experimented in a similar manner on frogs, and his results only differed from those of Letellier in this respect, that at a temperature as little reduced as from 35½° to 37½° Fahr., they fell into a torpid condition, in which they excreted an extremely minute quantity of carbonic acid (1000 grammes' weight of frogs yielding only 0·039 of a gramme in one hour), whilst they exhaled the greatest quantity (0·124 of a gramme) between 43° and 45° Fahr. As the temperature rose the quantity of carbonic acid gradually diminished, and between 82° and 84° the amount yielded by 1000 grammes' weight of frogs was only 0·077 of a gramme.

Vierordt has made a very extensive series of observations, with the view of ascertaining the connexion between the temperature and the pulse, the respiratory movements, and the volumes of expired air and of carbonic acid in one minute. His experiments were made at every degree of temperature between 37° and 76° Fahr. We shall place the mean results in two parallel columns, the first having reference to temperatures from 37° to 55°, and the second from 56° to 76°.

Mean Temperature.	47°	67°	Difference.
Pulse in one minute . .	72·93	71·29	... 1·64
Respiration in one minute	12·16	11·57	... 0·59
Volume of one expiration	33·5 cub. in.	31·8 cub. in.	... 1·7 cub. in.
Air expired in one minute	407·0 „	367·2 „	... 40 „
Carbonic acid expired in one minute . . .	18·3 „	... 15·7 „	... 2·6 „
Carbonic acid in 100 parts of expired air	4·48	... 4·28	... 0·20

From this table we see that elevation of the temperature is accompanied by a diminution of the number and of the depth of the inspirations; its effect upon the excretion of carbonic acid is, to a certain degree, of an indirect nature, since the diminished number and depth of the expirations must have a considerable influence; but inasmuch as the diminution does

* Comptes Rendus, vol. xx. p. 794.

not merely show itself in the total quantity excreted, but also in the per centage of carbonic acid in the expired air, it seems obvious that the elevation of temperature has a more direct influence on the excretion of carbonic acid than could be accounted for by the modified respiratory action.

The degree of atmospheric moisture to a certain degree influences the respiratory functions and the excretion of carbonic acid. Lehmann, some years ago, experimented on this subject with pigeons, greenfinches, and rabbits. The quantity of carbonic acid exhaled in a moist air was much greater than that in a drier atmosphere. For example, 1000 grammes' weight of pigeons yielded in one hour, in *dry* air, 10·438 grammes at 32° Fahr., 6·055 at 75°, and 4·69 at about 100°; while in a *moist* atmosphere, they yielded 6·769 grammes at 73°, and 7·176 at 100°. The other animals yielded similar results; and although the experiments were not very numerous, they are sufficient to show that there is a connexion between the degree of moisture of the inspired air and the quantity of carbonic acid excreted, although we cannot clearly see the reason of it.

The researches of Vierordt on the influence of atmospheric pressure, show that this also is by no means an unimportant element. The following numbers are extracted from his tables:

Barometer.	Pulse.	Volume of one expiration.	Air in one minute.	Carb. acid in one minute.	Per cent. of carbonic acid.
27 in. 8 lines	70·9	528·6 cc.*	6721 cc.	272·51 cc.	4·150
28 „ 2	72·2	529·2 „	6607 „	271·16 „	4·141

The influence—real or apparent—of different periods of the day was carefully examined by our distinguished countryman, Prout, and has subsequently been made the subject of experiment by Scharling, Vierordt, and Horn. Although these chemists have clearly shown that the products of respiration differ to no small extent at different periods of the day, there can, we think, be no doubt that these differences depend far more on the internal condition of the organism, as, for instance, on the digestion, on sleep, &c., than on mere cosmical conditions. Marchand was led to believe, from his earlier experiments on frogs, that there was a great difference between the daily and nightly secretion of carbonic acid; but on further investigation, and on eliminating certain causes of error, he arrived at the conclusion, that the influence of day and night is extremely trifling, and that any slight diminution during the latter period may be referred simply to the comparative quietude of the animals.

As might naturally be expected, abstinence from food exerts a very marked effect on the pulmonary excretion. Letellier found that while 1000 grammes' weight of turtle-doves, in their ordinary condition, exhaled in one hour 5·687 grammes of carbonic acid, they yielded only 4·120 grammes in an equal time, after a week's starvation. Boussingault has made similar experiments with a like result; a week's fasting reduces the hourly exhalation from 4·169 to 2·050 grammes. But, perhaps, the most carefully conducted experiments on this subject are those of Marchand on frogs. He found that these animals, when deprived of food, gradually exhaled less carbonic acid, and absorbed less oxygen; the ratio,

* We retain the French measure here, as the centimètres show the differences more distinctly than our own inches.

however, of the absorbed oxygen to the exhaled carbonic acid continued to rise till it reached about 420 : 100, the great excess of oxygen being clearly applied to the oxidation of the hydrogen; it afterwards fell to 300 : 100, and even to 270 : 100, so that there was scarcely sufficient oxygen absorbed for the formation of the carbonic acid; and this low ratio remained tolerably constant.

It appears, from the extensive researches of Regnault and Reiset, that the expired air during fasting presents nearly the same characters in the most varied classes of animals. The consumption of oxygen is invariably less in fasting than in well-fed animals: thus, for instance, 1000 grammes' weight of rabbits, when fasting, absorbed, on an average, only 0·749 of a gramme in an hour; while, after a full meal, the quantity was 0·877. In the fasting state, however, far less of the absorbed oxygen reappears in the carbonic acid than in the case when the animals are fed upon amylaceous food: thus, for instance, in rabbits fed upon carrots, from 84 to 95 per cent. of the absorbed oxygen is applied to the formation of carbonic acid; while, in the fasting state, the quantity does not exceed from 70 to 76·2 per cent. These observers very frequently noticed an absorption of nitrogen in fasting animals; in birds it was an almost constant phenomenon.

We may here notice two well-devised series of experiments which Bidder and Schmidt instituted, with the view of determining the modifications which the respiration undergoes during a total abstinence from food.

A cat, weighing 2464 grammes, exhaled, during an eighteen days' fast, 699·52 grammes of carbonic acid (= 190·78 grammes of carbon), and 525·67 grammes of aqueous vapour. It was shown by the quantitative determination and analysis of the other excretions, that (almost exactly in accordance with the direct observations of Regnault and Reiset) only 76·5 per cent. of the absorbed oxygen was again separated with the expired carbonic acid; further, that with every 100 parts of carbonic acid, 75·15 parts of aqueous vapour were exhaled, and that 41·72 per cent. of the whole water given off was removed by the perspiration. From the daily observations, it followed that the absorption of oxygen gradually diminished till the animal died, at first rapidly, then more slowly till the thirteenth day, and then again more rapidly till death ensued. The quantity of the expired oxygen which was not applied to the formation of carbonic acid, at first diminished with considerable rapidity, but afterwards more slowly and uniformly. At the beginning of the experiment, 80 per cent. went to form carbonic acid; on the second day, 77·4 per cent.; and for some time before the end, only 73 per cent. The quantity of carbonic acid excreted in twenty-four hours, diminished with nearly uniform rapidity for the first six days; in the next six days, only slowly; and in the last six days, again more rapidly. The daily amount of the exhaled aqueous vapour sank slowly and tolerably regularly; it being somewhat accelerated at the beginning and towards the close of the experiment.

In the second series of experiments (which were made on an adult cat, into whose stomach a large quantity of water had been injected), the ratio of the absorbed oxygen to that which was exhaled in the form of carbonic acid, was almost precisely the same as in the former case, there being 75·3 per cent. of oxygen thus employed; with every 100 parts of

carbonic acid 95·7 parts of aqueous vapour were exhaled; while in the previous case (where no water was allowed) for every 1000 grammes' weight of the animal there were daily exhaled 21·641 grammes of carbonic acid, and 16·281 of aqueous vapour: here (where water was abundantly given) the relative quantities of carbonic acid and water were 16·30 and 15·60 grammes respectively. Hence the water clearly impedes the loss of tissue.

It appears from certain experiments of Vierordt's that we cannot omit one of our ordinary meals without materially affecting the respiratory process. The experiments were made upon himself, and half-past twelve o'clock was his dinner-hour.

Time.	Pulse.	No. of respiration.	Vols. of one expiration.	Air in one minute.	Carb. acid in ditto.	Carb acid in 100 vols of expired air.
At 12 o'clock . .	63	10	545 cc.	5450cc	270·22cc.	4·69
At 2 o'clock, having dined	78·8	11·22	558·7 „	6162 „	307 36 „	4·74
At 2 o'clock, not having dined . .	62·5	9·5	575·0 „	5479 „	258·18 „	4·73
Differ. at 2 o'clock dependent on dinner	16·3	1 17	16·3 „*	683†	49 18‡	0 01

It is obvious from this table that, *cæteris paribus*, the respiratory functions are most energetic very shortly after a meal, and that they then gradually diminish in intensity. The only apparent exception is in the case of the volume of each expiration; but this may be readily explained on anatomico-mechanical grounds. Vierordt and Barral agree in the opinion that more carbonic acid is excreted in winter than in summer.

The effect of the chemical nature of the food on the respiratory products has been made the subject of inquiry by numerous chemists, amongst whom we may especially mention Dulong, Despretz, Lassaigne and Yvart, Letellier, and Regnault and Reiset. We must restrict ourselves to a notice of the results obtained by the last-mentioned investigators. They found that when dogs were fed on amylaceous food, far more oxygen was applied to the formation of carbonic acid than when they were fed upon flesh; in the latter case only 74·5 per cent. of the oxygen being thus used, while in the former the per-centage amounted to 91·3. A little nitrogen was exhaled during a vegetable diet, but far less than when flesh was eaten. A dog fed on mutton-suet neither exhaled nor absorbed nitrogen, and only 69·4 per cent. of the oxygen which it absorbed was returned as carbonic acid. Fowls, when fed on flesh after several days' starvation, absorbed a considerable quantity of nitrogen, but after they became accustomed to this abnormal diet, they returned to their ordinary habit of exhaling it; and, as in the case of the dogs, far less of the absorbed oxygen was returned as carbonic acid than when they were fed on their natural food; in two cases only 63 per cent. of the absorbed oxygen was contained in the carbonic acid which was exhaled. It further followed from these experiments, that after an animal diet the interchange of gases in the lungs is very similar to what occurs during fasting; and as similar observations have been made with reference to the urine and other excretions, it may perhaps be a legitimate inference, that a fasting animal to a certain degree lives upon its own flesh.

* Or 1 cubic inch. † Or 41·7 cubic inches. ‡ Or 3 cubic inches.

We shall give Lehmann's views on the question, how far the nature of the food influences the absorption of oxygen, and the excretion of carbonic acid, in his own words, with very slight abbreviations:

"We commence with a postulate (which will be subsequently established by inductive proof) that all the carbon and hydrogen of the fats and carbo-hydrates derived from the food, are completely oxidized in the living body into carbonic acid and water. It must be obvious to every one acquainted with the composition of these substances, that very different quantities of oxygen are required for their complete oxidation. The mean composition of the fats is about 78·13 C, 11·64 H, and 10·13 O: for the oxidation of the carbon (into carbonic acid) and of the hydrogen contained in 100 grammes of fat, there would be required 208·35 + 93·92, or 302·27 grammes of oxygen; but as the fat already contains 10·13 per cent. of oxygen, it would only require 292·14 grammes of that gas for its perfect combustion into carbonic acid and water. On comparing the composition of sugar with that of fat, we at once perceive that the carbo-hydrates require far less oxygen for their perfect oxidation than the fats; the former have no hydrogen to oxidize, as the oxygen they already contain is sufficient for this purpose; the carbon is, therefore, the only substance in them requiring oxidation, and it occurs in far smaller quantity in the carbo-hydrates than in fats. In the organic acids, as tartaric, citric, and malic acid—which occur in many articles of food—there is so large a proportion of oxygen, that it suffices not only for the oxidation of the hydrogen, but also of a portion of the carbon.

"In the case of nitrogenous food we cannot, however, admit the postulate, that all the carbon and hydrogen are consumed in the animal body; for we know that the greater part of the nitrogen in these substances is not liberated in the form of ammonia, but is eliminated in combination with carbon, hydrogen, and a little oxygen, by other channels than the lungs. Hence we are led to inquire, whether, and to what extent, the nitrogenous nutrient substances yield materials for oxidation, and consequently, how much carbonic acid and water they are able to furnish for respiration. As we have already seen that the albuminates and collagen* are capable of maintaining respiration, we are induced, in explanation of their value in this respect, provisionally to adopt the mere hypothesis, that these substances are disintegrated in the animal body only into carbonic acid, water, and urea, although we know that other nitrogenous products of excretion besides urea are formed; but since the urea preponderates very much in quantity over all similar compounds, and in some organisms—as, for instance, in the carnivora—occurs almost alone, this hypothesis is not altogether unworthy of notice in estimating the quantity of oxygen which is applied to the oxidation of the albuminates and of collagen: we therefore subtract from the composition of the albuminates, and other nitrogenous articles of food, a quantity of urea equivalent to their amount of nitrogen. If, for instance, we assume the composition of the albuminates, independently of their sulphur and salts, to be 54·36 C, 7·27 H, 16·05 N, and 22·32 O, there will remain, after the abstraction of the quantity of urea (=6·88 C, 2·29 H, and 9·18 O) equivalent to the (16·05 parts of) nitrogen from 100 parts of an albuminate, 47·48 parts of carbon, 4·98 of hydrogen, and 13·14 of oxygen. These relations will be more clearly seen in the following table:

Substance.	Carbon.	Hydrogen.	Oxygen.	Oxygen requisite for the formation of CO_2 and HO in addition to that present.
100 parts of fat	78·13	11·74	10·13	292·14
" starch	44·45	6·17	49·38	118·52
" sugar (C_{12} H_{12} O_{12})	40·00	6·66	53·34	106·67
" malic acid (C_4 H_2 O_4)	41·38	3·45	55·17	82·78
" albuminates	47·48	4·98	13·14	158·31
" collagen	42·82	4·47	13·39	135·56
" muscular substance (muscle-fibrin, collagen) according to C. Schmidt	46·10	4·72	13·88	147·04.†

* A word introduced recently into chemistry, to indicate the tissues yielding gelatin.—Rev.

† Physiol. Chem., vol. iii. pp. 312, 313.

Passing over Lehmann's observations on the intimate bearing which these proportions have upon the development of animal heat, and confining ourselves to the respiratory process, we obtain from the last column certain numerical relations which may be regarded as representing the respiratory equivalents. If, for instance, we assume that an organism requires, for the proper discharge of its vital functions, to absorb 100 grammes of oxygen in a given time, the following quantities of the abovementioned substances would be necessary to combine with that quantity of oxygen: 34·23 grammes of fat, 84·37 of starch, 93·75 of sugar, 120·80 of malic acid, 65·23 of an albuminate, 73·77 of collagen, or 68·01 of dry flesh. As every student of physiology is aware that no one of these substances would of itself preserve the system in a normal condition, and that it is still a scarcely solved problem in dietetics as to the proportions in which they should be combined, these numbers must obviously be regarded as mere relative estimates of the values of these kinds of food in connexion with the respiratory process.

The following experiments, made by C. Schmidt on one and the same animal (a cat), afford a good illustration of the extent to which the quantity of food modifies the respiratory process. The cat was kept in excellent condition, and lost no weight when receiving 142·41 grammes of flesh daily. On this allowance, it absorbed 60·14 grammes of oxygen, and exhaled 65·60 grammes of carbonic acid and 30·88 of water in twenty-four hours. When receiving 247·32 grammes of flesh daily, the corresponding quantities of oxygen, carbonic acid, and water, were 103·84, 113·52, and 47·86 grammes. Regnault and Reiset obtained similar results with herbivorous animals.

Like Prout, Vierordt has found that the excretion of carbonic acid is both absolutely and relatively diminished even by the moderate use of spirituous drinks; he likewise confirms Prout's statement, that the augmentation of carbonic acid, which naturally accompanies the process of digestion, is considerably lessened by the ingestion of alcoholic fluids.

The excretion of carbonic acid is very considerably diminished during sleep. This is most decisively proved by the experiments of Scharling, who found that a man, during one hour of the night, exhaled only 22·77 grammes, who, during an hour of the next day, immediately after a meal, exhaled 33·69 grammes; and who, in another case, found that the horary excretion of carbonic acid during the night and during the day was as 31·39 : 40·74. Lehmann, some years ago, published some experiments of a similar nature on birds (wood-pigeons): he found that, for every 1000 grammes' weight of birds, there was, on an average, 6·156 grammes of carbonic acid exhaled in an hour during the morning, while during the night the quantity was 4·950 grammes.

Regnault and Reiset have made some interesting observations on the remarkable influence which hybernation exerts on the respiratory process. They found that marmots absorbed, in their winter sleep, from 0·040 to 0·04[illegible] a gramme of oxygen in an hour to every 1000 grammes of their own [illegible]; while in their summer state, when awake, they consumed from [illegible]4 to 1·198 grammes. In their sleeping condition, only 56·7 per cent. of the absorbed oxygen was found in the carbonic acid; while in their active state, it amounted to about 73 per cent. In two out of three experiments,

they observed in the hybernating animals a considerable absorption of nitrogen; whilst in their active state, like other animals, they exhaled this gas. This excessive absorption of oxygen, and considerable absorption of nitrogen, explain the fact first noticed by Saco, that marmots frequently increase in weight during their hybernation. They obtained similar results with lizards, which were reduced to a state of torpor by the action of cold.

Prout, and subsequently Vierordt, showed that in man, immediately after waking, there is a considerably augmented excretion of carbonic acid; as a parallel experiment, Regnault and Reiset found that their marmots, on first awakening in the spring, exhaled very large quantities of carbonic acid, and consumed more oxygen than after they had become entirely active.

"That age," says Lehmann, "is not without its influence on the respiratory process, is proved by experiments both on men and animals. Andral and Gavarret have instituted tolerably extensive observations on the absolute quantity of carbonic acid that is exhaled; from which it follows, that the daily amount increases, on an average, to the 40th or 45th year, varying in accordance with the development of the muscular system. In Scharling's experiments, the two children that he employed (a boy aged nine years and three-quarters, and a girl of ten) exhaled nearly double the quantity of carbonic acid, in relation to their weight, that adults did; excluding, however, this reference to weight, there is a perfect coincidence between Scharling's results and those of Andral and Gavarret. The observations made by Regnault and Reiset on animals are in accordance with those made on man. They found that in animals of the same species the young consumed more oxygen than the adult.

"With regard to the influence of sex upon respiration, Scharling's observations, as well as those of Andral and Gavarret, show that males exhale more carbonic acid than females; a law which holds good even in childhood, boys producing more carbonic acid than girls.

"As the results of Scharling's investigations must be regarded as giving us (at all events for the present) the standard numbers for the normal excretion of carbonic acid in man, we give those numbers here, calculated for one hour, and reduced from (Danish) grains to grammes.

Subject.	Age.	Weight of body.	CO_2 in 1 hour.	Ditto calculated for 1000 grammes.
	Years.	Kilog.*	Grammes.	Grammes.
Man	35	65·50	33·530	0·5119
Youth	16	57·75	34·280	0·5887
Soldier	28	82·00	36·632	0·4466
Maiden	17	55·75	25·342	0·4546
Boy	9¾	22·00	20·338	0·9245
Girl	10	23·00	19·162	0·8831

"According to Andral and Gavarret, an adult man exhales, on an average, from 38·5 to 40·3 grammes in an hour; an adult female, when not pregnant, from 22·0 to [illegible] grammes, during pregnancy 29·3 grammes, and after the cessation of menstruation from 27·5 to 31·2 grammes of carbonic acid. Although Scharling includes the products of perspiration, yet Andral and Gavarret's numbers are the highest; this apparent anomaly may be probably explained by supposing that in their experiments the respirations were more laboured, and, at all events, more frequent, than in the corresponding experiments of Scharling, which were instituted on persons enclosed in a large and commodious respiratory chamber (*Respirationsbehältniss*)."†

* The kilogramme = 1000 grammes = 2·2lbs. nearly.

† Physiol. Chem., vol. iii. pp. 319-20.

We now arrive at that part of Lehmann's chapter in which he discusses the chemistry of the respiratory process in the lower animals. Passing over, for want of space, his remarks on this process in mammals, birds (and their eggs), reptiles, and insects, we shall give a brief abstract of some interesting experiments recently made by Baumert,* on the respiration of fishes. The subjects of his inquiry were the tench (cyprinus tinca), the gold-fish (cyprinus aureus), and the pond-loach (cobitis fossilis). He found, by means of an ingeniously constructed apparatus, that tench inspired, on an average, to 1000 grammes of their own weight, 0·0143 of a gramme of oxygen, and exhaled 0·0138 of a gramme of carbonic acid in one hour; while for the more active and lively gold-fishes the corresponding numbers, representing the oxygen and carbonic acid, were 0·0409 and 0·0419 of a gramme respectively. The volume of the absorbed oxygen is, to that of the exhaled carbonic acid, in about the ratio of 10·7; and of 100 parts (by weight) of the absorbed oxygen, 72·3 are returned with the carbonic acid. With the loach the results were somewhat different. This fish, in common, we believe, with a few others, possesses what may be termed an intestinal respiration, in addition to the ordinary gill-respiration. It may be very commonly seen ascending to the surface of the water, opening its mouth, and swallowing air, which is transmitted to the stomach and intestinal canal. Baumert has analyzed the air after it has passed through the intestines, and has found it to contain considerably less oxygen than when it was swallowed; and in the place of the oxygen, there is considerably less carbonic acid than we are accustomed to find in either branchial or pulmonic respiration. The oxygen absorbed by the intestine must, therefore, pass directly into the blood, while the carbonic acid which it contributes to form would seem to be separated by the gills. This view is, at all events, borne out by Baumert's observation, that in the branchial respiration of the loach there is exhaled far more carbonic acid in proportion to the inspired oxygen, than is given off in a corresponding manner by the tench or gold-fish.

The loaches were observed to swallow air, in the manner we have described, much more frequently in stagnant than fresh (and, therefore, well-oxygenized) water; and it seemed, from certain experiments made by Baumert, that they soon exhibited symptoms of disease if either of their respiratory functions were impeded. Lehmann seems to have overlooked the observations made many years ago by Ermann† on the intestinal respiration of the loach.

Our knowledge of the respiratory products in special diseases is still, and will probably long continue to be, in a very unsatisfactory state. In many acute diseases it would be obviously impossible to submit the patient to the necessary course of investigation. In connexion with this subject, Lehmann records certain experiments which he instituted on rabbits, in which he had artificially set up inflammation of the lungs and muscular tissue, and gives an abstract of the researches of Hervier and Saint Sager on the respiration in inflammatory disorders, of Hannover in chlorosis and pulmonary tuberculosis, of Doyère in cholera, and of Mal-

* Chem. Untersuch. über die Respiration des Schlammpeitzgers. Heidelberg, 1852.

† Quoted in Müller's Handbuch der Physiologie. See Baly's translation, second edition, vol. i. p. 331; originally published in vol. xxx. of Gilbert's Annalen.

colm on typhus. Isolated inquiries, such as these, can, however, be of little service to science; and we can hope for no trustworthy and available observations until chemists shall have so far agreed upon the methods of analysis that their results will bear comparison with one another.

Review XII.

Klinik der Unterleibs-Krankheiten. Von Eduard Henoch, Doctor der Medicin und Chirurgie, und Privat-Docenten an der Friedrich-Wilhelms Universitätzu, Berlin. Erster Band. — *Berlin*, 1852, pp. 303.

A Clinical Treatise on the Diseases of the Abdomen. By Edward Henoch, Doctor of Medicine and Surgery, &c. First Volume.—*Berlin*, 1852. pp. 303.

The plan which Dr. Henoch has adopted in the work before us is one which we would gladly see more generally introduced into our professedly practical literature. He treats of diseases as met with at the bed side, classifying them less according to the organs which they affect, than according to the prominent symptoms which they exhibit. This method is identical with that which is adopted in the clinical instruction given in hospitals, where cases are grouped together which resemble one another in some more prominent characters, with a view to point out the way in which they are to be distinguished from one another, and the reason of the different modes of treatment demanded by each. But while we see in this arrangement a great practical end gained, the objection arises that certain diseases are from time to time separated which, though disagreeing in their prominent symptoms, agree in affecting the same organ, or even the same part of an organ or its appendages. But where the aim of the work, as that of Dr. Henoch's, is purely practical, this is of little real moment, since the diseases omitted find their place (and clinically, their natural and proper place) amongst those perhaps of other organs, which they resemble in their symptoms, and with which there is consequently a possibility of their being confounded.

Dr. Henoch devotes the early pages of his book to a brief discussion of some of the most obvious signs of disease apparent upon inspection, handling, and percussion of the abdomen; and then, seizing upon the presence of firm tumour as a prominent sign of disease, and consequently one which cannot be overlooked in making a diagnosis, he proceeds to give a brief clinical account of those affections which, not being, strictly speaking, diseases of the organs of the abdomen, are nevertheless capable of simulating them by producing abdominal tumours. We will follow him through his observations upon these diseases.

Fæcal Tumours it is well known have frequently been mistaken for enlargements of abdominal organs, of the liver, kidney, ovary, uterus, &c. In illustration of these errors, Dr. Henoch transcribes a case from Andral, where a tumour, which was taken by that observer for a tumour of the liver, manifested its true nature by being dispersed, with all its accompanying symptoms, on diarrhœa being induced by a dose of taraxacum juice. He alludes also to one of his own observations, in which gall-

stones were at one time suspected; there were present in this case great enlargement of the liver, with tenderness, bilious vomiting, and fever; while close above the navel, where, considering the size of the liver, the gall-bladder might have been expected to lie, there were perceived, apparently immediately beneath the abdominal wall, four or five small hard somewhat moveable tumours, which however disappeared on free evacuation of the bowels. In cases of fæcal retention of this kind, we must not always expect to meet with complete constipation, since it is known that even diarrhœa may be present, with great and serious fæcal accumulation: jaundice may also arise in consequence of the fæcal masses exercising more or less pressure upon the choledic duct.

Peritoneal Abscess.—Dr. Henoch relates a case in which the first idea was, that an excessively tender tumour, situated in the right lateral region of the abdomen, and accompanied by vomiting, was an enlarged kidney; but the absence of all the symptoms of renal disease discountenanced this view, and the patient recovered completely, with total disappearance of the tumour, in the course of a few weeks. He also refers to two cases by Andral, in one of which a collection of pus, enclosed by thick false membrane, formed during life a tumour below the margin of the ribs on the left side, extending behind them, and simulating an enlarged spleen; while in the other, a similar accumulation in front of the stomach produced a projection in the epigastrium, such as is accustomed to result from a tumour of a subjacent organ.

Pelvic Abscess, which has at various times been mistaken for disease of the liver, kidneys, ovaries, &c., or for fæcal tumours, chiefly occurs in females after delivery; but, putting parturition out of the question, Grisolle has shown that the disease is by far more frequent in the male than in the female sex. Out of 73 cases which he collected, 17 occurred after delivery, and of the remaining 56, only 10 were observed in females. The causes of the disease, which our author illustrates by reference to cases, are, in addition to a recent confinement, mechanical injury, such as may arise from sudden muscular effort in straightening the body, as a result of which some fibres of the psoas may be ruptured, and a portion of its investing cellular tissue torn; the presence of suppurating bubo; disease of the prostate gland, or of the urethra; and inflammation of the spermatic cord. In these cases the suppuration mostly occurs in the cellular tissue, between the peritoneum and the fascia, more rarely beneath the fascia, or in the cavity of the peritoneum. In distinguishing pelvic abscess from cystic disease of the ovary, a diagnosis which the position of the tumour often renders necessary, Dr. Henoch properly insists upon the great importance of keeping clearly in view the early history of the affection. Encysted disease of the ovary nearly always commences without any striking morbid phenomena, whilst pelvic abscess is ushered in by marked symptoms of acute inflammation. Among these may be enumerated fever, cold shivering, vomiting, sweats, local pain, and tenderne[illegible] Extension of the thigh and sudden movements of the trunk are apt [illegible]crease the pain; and Dr. Henoch has observed in addition, that t[illegible]g may become shortened, and consequent lameness be induced, by [illegible]raction of the psoas and iliacus muscles, especially in those instances in [illegible]hich the abscess, situated behind the fascia iliaca, compresses and irritates

the muscles. On this ground he dissents from the view which regards this symptom as one by which psoitis may be distinguished from the disease under consideration. In the diagnosis of pelvic abscess, also, he places little reliance upon a sign commonly much depended upon—the detection of fluctuation. He states, that in the cases which have come under his observation, he has always failed to perceive it; and refers to two cases of large pelvic abscess related by Bourienne, where not the slightest fluctuation was present. Among the errors into which the form of a tumour may lead the practitioner, he specially mentions that of mistaking it for an acute inflammation of the right lobe of the liver, as was once observed by Dupuytren. He also relates a case which he observed himself, in which a tumour of the left ischium might have been confounded with pelvic abscess.

Tumours of the Peritoneum, and especially of the large or small omentum, or of the mesentery, may readily simulate disease of the abdominal viscera, as of the liver and uterus, instances of which are referred to in the writings of Andral and Bright. One of the results of chronic peritonitis, familiar to us from the description given of it by Dr. Hodgkin, is the shrinking up of the large omentum, so as to form a mass beneath the great curvature of the stomach. When this mass is further thickened by the deposition of the products of inflammation, tubercle, or cancer, it may give rise during life to palpable tumour. Dr. Henoch relates a case at great length in connexion with this fact, and illustrative of the diagnostic errors which may arise out of it.

Tumours of the Organs of the Abdomen.—Dr. Henoch takes up, in the first place, the *diseases of the liver*, of which enlargement or tumour is a prominent indication. The clinical description of these diseases forms the mass of the volume under review. Before entering, however, upon the subject of the true diseases of the liver, the reader is very properly warned, that it is not enlargement of the organ alone which can give rise to palpable tumour below the margin of the ribs, or to extended dulness upon percussion. Causes acting upon the liver from without, such as a habit of tight-lacing, may so flatten and lengthen the organ, and contract the thoracic space, as to cause the exhibition of both of these physical signs of enlargement. Diseases in parts adjacent to the liver may also have the effect of displacing it, and of pushing it downwards, upwards, outwards, or laterally, according to the structures they affect, and the amount of pressure they exert. In distinguishing *dislocation of the liver* downwards, in consequence of pleuritic effusion from enlargement of the organ by disease, Dr. Stokes relies, among other signs, upon the presence of a furrow, which may be perceived between the diaphragm and convex surface of the liver. Dr. Henoch, however, asserts that he has noticed the same sign in cases of cancerous enlargement of the liver, where he has been able, without difficulty, to introduce the fingers under the margin of the ribs, and, therefore, between them and the enlarged liver. The descent of the liver on deep inspiration, a sign used by Dr. Stokes to distinguish enlargement of the liver from its dislocation by empyema, is regarded

"Of especial value where the liver has enlarged upwards towards the right cavity of the chest, a state of things which may easily give rise to the error of

supposing empyema to be present, on account of the great upward extent of the dulness on percussion. But whilst, in the presence of the latter disease, deep inspiration is without effect, in enlargement of the liver, the upper limit of the percussion-dulness becomes lowered." (p. 59.)

Hyperæmia and Inflammation of the Liver are the first of the pathological conditions noticed as productive of enlargement of the organ. After alluding to the usual causes of *mechanical* hyperæmia, such as those diseases which impede the pulmonary circulation, or produce over-fulness of the right side of the heart, Dr. Henoch mentions, with a view to criticising it, the assumption of Bright, that an overloaded state of the bowels may occasion congestion of the liver, by exercising pressure upon the returning veins of the organ. The only vein returning blood from the liver is the hepatic vein, and it is not easily perceptible how this can suffer compression from the cause referred to.

Our own experience fully confirms the recommendation of our author, in the treatment of hyperæmia of the liver, to apply leeches to the anus, in place of withdrawing blood from the surface of the body over the hepatic region. One great object gained is, that through the free anastomosis of the systemic and portal vessels in this situation, the latter are subjected to little short of direct depletion; and another is, that a very moderate loss of blood is equally efficient with a much larger quantity, when abstracted from any other part. The recent experience of Haspel and others in Algeria, confirms, in a striking manner, the opinion of the older physicians as to the beneficial results which sometimes follow the application of blisters to the region of the liver in the treatment of hyperæmia and inflammation.

Except after traumatic injuries, acute inflammation of the liver rarely terminates in suppuration. The passage of hyperæmia into *acute inflammation* is not marked by any certain signs, unless we choose to regard, as betokening inflammation in any given case, an unusual amount of fever, heat of skin, thirst, bilious vomiting, and short cough; and fortunately as respects treatment, the diagnosis is immaterial. Jaundice, which occurs sometimes in simple hyperæmia, is often wanting in true inflammation. Oppolzer only met with it in twenty-five out of fifty-five cases, and then, indeed, only when the inflamed part directly compressed the bile-ducts, or when the latter were themselves obstructed by exudation. The pain also is not usually severe, unless the peritoneum covering the liver is involved in the inflammation, and the pulse, as long ago shown, may not exceed the normal rapidity, and may even fall below it.

Inflammation of the Gall-bladder or of the Bile-ducts may produce a series of symptoms similar to those arising from inflammation of the parenchyma of the liver. *Acute idiopathic inflammation of the gall-bladder* is a very rare disease. Two cases, recorded by Dr. Graves in the 'Dublin Journal,' are familiar to the profession, in which local pain, nausea, vomiting, and fever were present, without any extended dulness on percussion, or swelling in the region of the liver. Dr. Henoch refuses to admit, on the ground of insufficient proof, the diagnostic value of a limitation of the tenderness to a small spot between the epigastrium and hypochondrium; nor does he admit the possibility of a pear-shaped tumour being formed by the accumulation of the products of inflamma-

tion within the gall-bladder, so long as the ducts are permeable. *Inflammation of the common bile-duct* is more common, and often arises from extension of inflammation into that canal from the mucous membrane of the duodenum; and the swelling of the membrane results in diminished calibre or occlusion of its canal. *Secondary inflammation of the mucous membrane of the gall-bladder*, with ulceration, however, is of no unusual occurrence. Its causes, as enumerated and discussed by our author, are —1. Mechanical irritation of the membrane from the presence of gall-stones; 2. A morbid condition of the bile; and 3, as associated with the latter, a permanent closure or narrowing of the cystic duct.

Dr. Henoch attributes great importance to the natural *cessation of the catamenia* or their accidental arrest as a cause of hyperæmia of the liver, and believes that the hyperæmia thus induced may lay the foundation of further degeneration of the organ at some future period. In illustration of this etiological relation, he adduces a case in which the liver, greatly enlarged from a prolonged ague, diminished in volume on the occurrence of menorrhagia; but on exposure to cold some months afterwards, the catamenia present at that time became repressed, the enlargement of the liver returned, and ultimately cancer of the liver was clearly established.

Another important cause of hyperæmia is *atmospheric heat*, which operates far more remarkably in tropical countries than in our own temperate latitudes.

"I believe that all the symptoms which physicians practising in the tropics, as, for instance, Annesley, refer to a functional disturbance of the liver, 'an excessive secretion of bile,' are dependent originally and principally upon an increased fulness of the liver with blood, which becomes very frequently associated with similar hyperæmia of the mucous membrane of the intestines. We know how frequently this latter hyperæmia occurs amongst ourselves in the hot months of summer, as the cause of profuse diarrhœas; and although the green colour of the evacuations by no means proves, as is too commonly supposed, their actual bilious character, yet symptoms often arise, especially in hot climates, which, in fact, demonstrate a participation of the liver in the hyperæmia." (p. 84.)

This hyperæmia, with diarrhœa or dysentery, is known to be endemic in tropical countries, and presents the characters of "bilious remittent fever." In this instance, however, the *influence of miasmata* must not be excluded from consideration; since, amongst other evidence, Haspel noticed that, in Oran, during the summer of 1846, when the heat was so great, as to dry up the marshes, affections of the liver were absolutely lessened in frequency.

A result of this hyperæmia when excessive, is that *softening of the liver from hæmorrhagic infiltration*, which medical practitioners have long known as occurring in the malignant fevers of Italy and India.

But the blood is not the only liquid whose impeded flow out of the liver, or accumulation within it, may give rise to enlargement. The same phenomenon may arise from the *retention of the secreted bile*, and its accumulation in the ducts within the organ. Some remarks on this retention of bile, or *biliary congestion*, form, consequently, a natural appendix to the subjects just discussed. The time at which it occurs, as a result of impeded flow of bile from the duct, is earlier or later, according as the obstructing

cause is situated above or below the mouth of the cystic duct. If it be below this point, the cystic duct and gall bladder become first of all distended with the accumulated secretion, and the tumour arising from the accumulation is mostly distinguishable by palpation. The detection of an enlarged gall-bladder, however, must not be too rashly used as positive evidence that the obstruction is in the common duct, and not in the hepatic; since this sign may arise not only from accumulation of bile, but also from a collection of mucous or serous fluid, which forms the "hydrops cystidis felleæ" of authors. In all cases, retention of bile is sooner or later accompanied by *jaundice*. A very important diagnostic feature of this affection is, that, after a time, the enlarged liver gradually diminishes in bulk, notwithstanding that all the remaining symptoms of the disease are unaffected, or become more marked in their character. The pressure of the retained secretion exerts its influence upon the nutrition of the essential elements of the organ; the capillary circulation becomes injured, and the vessels themselves obliterated, while the cells become fatty, and finally disappear. The necessary consequence is atrophy of the substance of the liver, similar to that which occurs in the kidney in consequence of obstruction of the ureter. A case is related, as an example of this condition, in which the liver, at first distinctly enlarged, was found, after death, reduced to one-fourth of its normal volume.

Dr. Henoch ascribes much diagnostic importance to *emaciation*, which was very remarkable in this instance—since even in cancer of the liver it is not a very prominent symptom. It is, however, proper, before admitting the great importance claimed for this symptom, that we should satisfy ourselves, in the cases where emaciation has been excessive, that the pancreatic duct has not been equally occluded with the choledic. In a case which very lately came under our observation, in which retention of bile was present in consequence of the encroachment of cancerous disease about the portal vessels upon the bile ducts, there was the most extreme emaciation; but the pancreatic duct was, in this instance, also occluded, and the gland distended with the accumulated secretion. In cases of this *secondary atrophy of the liver*, hæmorrhage from the stomach and intestines may appear, being partly due to the injury done to the capillary circulation of the liver, and partly to a morbid condition of the blood. The special cause of obstruction in the duct is beyond the reach of certain diagnosis; in cases, however, where a patient has suffered at a former period from gall-stones, the impaction of a gall-stone in the duct may be suspected; while the occurrence of fatty matters in the stools, when but little fat has been taken in the food, may perhaps lead to the belief that the obstruction has arisen from the pressure of a morbidly enlarged pancreas, or an affection of the duodenum.

The treatment of biliary congestion consists in the removal, where possible, of the cause of obstruction. Where dependent upon inflammatory swelling of the mucous membrane of the duodenum or choledic duct, this is to be attempted by the judicious use of antiphlogistic remedies. Where, however, secondary atrophy of the liver has occurred, no hope of cure can be reasonably entertained, and the only remedy suggested by our author is a trial of the alkaline mineral waters of

Marienbad or Carlsbad, the use of which is especially recommended where the suspected cause of the impermeability is an impacted gall-stone.

Fatty Liver is the next condition discussed. Fat may be found in the liver constituting its sole structural change, or it may form one of a series of important alterations which the organ has undergone from inflammation, cirrhosis, cancer, &c. In the former case it is the primary, in the latter a secondary affection. Gluge, however, has included all the conditions of the liver where fat is present under one disease, which he has termed "stearosis," considering the nutmeg liver, fatty liver, lardaceous liver, and cirrhosis, as merely different degrees of development of one and the same disease. Reinhardt, on the other hand, regards the presence of fat as the result of a retrograde nutrition, which occurs with the greater facility under the influence of hyperæmia or inflammation, in which case it is secondary to these affections.

The primary form of fatty liver—*stearosis proper*—gives rise to no symptoms of importance; even tumour is rarely ascertained, except in the last stage, and the margin of the liver, though mostly rounded, sometimes retains its natural sharpness. In the absence of symptoms, the diagnosis must be assisted by a consideration of the circumstances under which the disease is known to occur,—namely, in phthisical patients, and in indolent gourmands, especially in those who take much fatty matter in their food, and indulge in alcoholic potations. When occurring in phthisical individuals, it is more frequently observed, according to Home and Louis, in females than in males; and, according to Dr. Budd, we may assume its presence whenever, in a consumptive female, a considerable painless enlargement of the liver is observed without accompanying ascites. When arising in indolence and over-feeding, it is usually conjoined with a simultaneous increase of the subcutaneous fat. The physiological experiments of Magendie, Gluge, and Thiernesse, have shown how fatty liver may be produced artificially in animals by feeding them with fat, or by injecting it into the vessels; and Gulliver and Virchow state that the fat is always first deposited in the immediate vicinity of the branches of the portal vein. The treatment of the disease, when arising from this cause, consists in the avoidance of fatty matters in the food, and of fermented liquors, in early rising and exercise, and in the use of alkalies and alkaline mineral waters.

In that form of fatty liver, however, which complicates phthisis and other diseases, the resources of art are powerless.

Among the diseases with which it has been noticed as associated, but with too little constancy to assist in its diagnosis, may be mentioned, carcinoma, various diseases producing emaciation, chronic diarrhœa, and dysentery. Rilliet and Barthez have observed it in children, quite apart from tuberculosis, after smallpox, measles, scarlet fever, and typhus; Biett and Rayer in chronic pemphigus; and Bright in a case where death had been occasioned by an extensive burn.

Hypertrophy of the Liver—meaning, by this term, an increased nutrition of the organ, or in other words, an increased deposition of the normal substance properly belonging to it—is briefly dismissed by our author: the conclusion he arrives at is one in which we completely coincide.

"I cannot regard this simple hypertrophy of the liver as an object for diagnosis by the physician, and, until convinced of the contrary by authentic observations, consider that, in all cases in which, after important symptoms during life, only simple hypertrophy of the liver has been discovered, an error has been committed either in consequence of imperfect knowledge or superficial examination." (p. 122.)

Chronic Inflammation of the Liver is a far more important pathological condition. Its great anatomical characteristic is the occurrence of exudation as a result of hyperæmia, which exudation may take place into various tissues of the organ, and undergo various subsequent changes. Hence Dr. Henoch enumerates, as the results of chronic inflammation, *induration*, where the exudation into the general parenchyma of the liver becomes converted into a connective tissue; *cirrhosis*, where the inflammatory exudation into the prolongations of the capsule of Glisson, within the liver, undergoes a similar change; *abscess of the liver*, where the exudation becomes converted into pus; and *lardaceous liver*, where it neither forms pus, nor yet becomes changed into connective tissue, but continues much in the same amorphous condition in which it was originally deposited, being only partially converted into fat. The researches of pathologists, however, are daily tending to the abridgment of the domain of "chronic *inflammation*," and we think that our author, by referring these alterations of the liver at all times to inflammation has made too free an use of the term. Hyperæmia is essential to our idea of inflammation, and there is little doubt that it is not a necessary element in some forms of these diseases, which are consequently to be regarded rather as instances of degraded nutrition. Cirrhosis is not the only form of disease in which the *granular* character is remarked, since, although most common here, it is met with also, more or less frequently, in all chronic diseases of the organ.

Grouping together, then, induration, cirrhosis, and lardaceous liver, as only different forms of chronic inflammation or degraded nutrition, it is found that, notwithstanding the variety of the seat and subsequent changes of the exudation, the symptoms of the early stage of all, while the proper inflammatory process (?) is in operation and the exudation is being deposited, are alike. They are chiefly those known as "dyspeptic," and are not uncommonly treated as such, at first, by bitters and alcoholic stimulants. The occurrence of yellowness of the skin, or of some hepatic tenderness, however, after a time, directs attention to the liver, when the hypochondrium is found full and tender, and the liver, as ascertained by palpation and percussion, enlarged. In this stage it is possible to confound it with duodenal catarrh associated with biliary congestion, or with the early stage of cancer of the liver: from the latter it is not, at this stage, in [illegible] power to distinguish it, except by taking various concomitant cir[illegible]tances into consideration; from the former it may be distinguished by [illegible]act, that the jaundice is less marked than where the bile is retained [illegible] the canals of the liver.

[illegible] second stage is marked by obliteration of many of the bloodvessels, [illegible] a consequent impediment to the flow of the portal blood through the [illegible]r, which further acts in a backward direction, producing unnatural [illegible]lness, not only of the trunk of the portal vein, but of all those veins of

the stomach, intestines, spleen, &c., from which it springs. The results of this hinderance to the portal circulation, and of a simultaneous closure of some of the bile-ducts, constitute the leading symptoms of this stage.

These symptoms may be best studied in *cirrhosis*, limiting the term strictly to chronic inflammation of, or organising and contractile exudation into the prolongations of the capsule of Glisson. The congestion of the portal system manifests itself first by dropsical effusion into the peritoneum; and although ascites is, as a rule, the first of the dropsical phenomena in any given case, exceptions are not wanting. But not only does the venous fulness lead to effusion, but it also prevents absorption of liquids by the mucous membrane of the alimentary canal; and hence, according to Dr. Budd, arises the dryness and roughness of the skin without fever, the thirst, and concentrated urine, noticed in this disease. The fulness of the vessels may terminate also in hæmorrhage, either from the stomach or intestines. The impeded return of the blood through the liver gives rise to its seeking another course to the heart; hence enlargement of the superficial veins of the abdomen and chest, especially on the right side: this symptom, however, as shown by a case related by Dr. Budd, may be wanting even in extreme cirrhosis. Sometimes the portal fulness is indicated in addition by enlargement of the spleen, as manifested by extended percussion-dulness. When present, this is a valuable sign of cirrhosis, but its absence must not be regarded as opposed to the idea of the existence of this disease. Slight jaundice, with deficient colour in the stools, is due to the compression to which some of the smaller bile-ducts within the liver are subjected during the contraction of the exudation matter; complete jaundice is rare.

Obliteration of the Portal Vein, of its trunk, or of its branches within the liver, usually regarded as the result of adhesive inflammation, when extensive or involving the trunk or larger branches, arrests the circulation in the liver just in the same way as cirrhosis, and produces similar semeiological phenomena; the diagnosis being rendered the more difficult by the atrophy of the organ, which may result from the imperfect supply of blood.

In distinguishing between induration, lardaceous liver, and cirrhosis, which thus so closely agree in their symptoms, we must take into consideration the fact that the two former usually present considerable enlargement of the liver, which is smooth to the feel, and little, if at all, tender; while in cirrhosis, the liver is reduced in size, and not only is tumour wanting, but the diminished bulk of the liver may even sometimes be exhibited by percussion. A study of the etiological relations of these three affections may greatly assist in their diagnosis. The causes of *induration* of the liver are little known; it sometimes arises in previously healthy persons without any ascertainable cause. If we assume the "atrophy of the liver," described by Haspel, to be identical with the induration here referred to, it may sometimes be referred to a preceding dysentery, to a repetition of attacks of intermittent fever, and the influence of a hot and unhealthy climate. Dittrich has also described a similar disease, and shows its dependence upon the presence of syphilis.

Lardaceous Liver is the result of a dyscrasia, and is most frequently met with in scrofulous subjects. Dr. Graves considers that a state of the general health resembling scrofula may follow upon syphilis and the abuse

of mercury, which may give rise, also, to a similar affection of the liver; and Dr. Henoch believes, for reasons which he assigns, that probably the abuse of mercury alone without previous syphilis, may suffice to originate it. The influence both of mercury and of syphilis in this respect, however, clearly demands further investigation. The last cause mentioned by Dr. Henoch is protracted ague, but he offers no evidence in support of the influence of this disease.

Cirrhosis is well known to be of most frequent occurrence in tipplers, the alcohol carried by the portal veins into the liver permeating their walls, and acting as a local irritant. That this is the mode in which the alcohol acts is rendered probable by the seat of the alteration being the tissue which immediately surrounds the branches of the portal vein. But since it is observed also in temperate persons, some other possible causes must be assumed, and Dr. Budd suggests that they may consist in some other irritant matters carried by the portal vein to the liver, these being either some of the ingesta, or products of faulty digestion. The concurrence of disease of the heart, dwelt upon so fully by Becquerel, if not as effective as he supposes, may nevertheless be well believed to assist materially other influences in the production of cirrhosis.

The treatment of these several forms of chronic inflammation, as described by Dr. Henoch, requires but few remarks. In the first stage, it consists in the usual means of depleting the liver, such as the application of leeches to the anus, or the withdrawal of blood by leeches or cupping from the hypochondrium, blistering, derivation to the intestinal canal by small doses of calomel, rhubarb, aloes, &c., an antiphlogistic diet, and the avoidance of fatty articles of food, and of alcoholic drinks. Where syphilis is present, the remedies for this condition are demanded. In the second stage, only palliative remedies are admissible, the bowels being kept free by mild aperients, and the digestion assisted by bitters. Paracentesis for the relief of the ascites Dr. Henoch considers should only be had recourse to where the exigency is extreme, since the liquid very quickly re-accumulates, and the repeated withdrawal of so large a mass of serum may induce rapid sinking of the strength, and accelerate dissolution. Where the disease has not yet arrived at the last fatal stage, he considers that the use of the nitro-hydrochloric acid deserves a special recommendation. He regards the re-absorption of the exudation as the more likely to take place, and consequently the prognosis to be more favourable in the lardaceous liver than in induration or cirrhosis. When arising out of the aguish cachexia, quinine and the preparations of iron are to be recommended, and in scrofulous constitutions, iodide of potassium or iodide of iron, with frictions of iodine externally.

Abscess of the Liver is sometimes completely *latent*, none of the symptoms which ordinarily distinguish its development being present. Instances of this have been observed in our own latitude, as well as in the tropics. In other cases, the predominance of pulmonary symptoms has led to its being mistaken for phthisis, an error which, in one of Haspel's cases, was the more readily fallen into, since pneumonia at the apex of the left lung occasioned some of the physical signs of that state. Mostly, however, symptoms are present sufficient to characterise the disease. One of these is enlargement of the organ, which, "where the abscesses are deeply seated, is dependent less upon the purulent accumu-

lation itself, than upon the surrounding hyperæmia and exudation, perhaps also upon a partial retention of bile in the small ducts." (p. 163.)

Fluctuation may be wanting throughout the whole course of the disease, when the pus lies deeply in the parenchyma, but is more likely to be perceived when an abscess seated near the surface, especially of the lower part of the convexity of the liver, is making its way outwards towards the intercostal spaces, or towards the abdominal wall below the margin of the ribs; but even under these circumstances it may be obscured by tension of the muscles. Pain, or some uneasy sensation, is almost always observed. Complete jaundice rarely occurs, and when present, it results not from the mere extent of the suppuration, but from retention of bile within the liver, occasioned either by the larger bile ducts being compressed by the abscess, or the smaller ones within the liver being similarly obstructed. The consensual symptoms, pain in the right shoulder, tension of the right rectus muscle, vomiting and cough, any of which may lead to errors of diagnosis, are little to be relied upon as characters of abscess of the liver, since they occur also in very different diseases of the organ. Abscess of the liver from chronic inflammation may pass through its entire course without fever; in most patients the pulse is small, rather slow than frequent, and only becomes rapid during the prostration which precedes the fatal termination. Hectic fever also is less common than has been supposed. M. C. Broussais, in 42 cases, only observed rigors 20 times, and night-sweats 4 times. The emaciation and exhaustion of strength, rarely wanting, are attributable not merely to the suppuration, but to the diseases which accompany it, especially to the intestinal disease.

The connexion between abscess of the liver and dysentery, as a clinical fact, is indisputable. Dr. Henoch, however, refuses his assent to the explanation of it, supported by Budd, who views it as a result of phlebitis, the pus from which, carried by the portal blood, is arrested in the liver, and becomes the focus for the formation of an abscess. Apart from the debated question of the capability of the inner membrane of veins undergoing inflammation at all, Dr. Henoch considers, as opposed to this explanation, the fact that Dr. Parkes, on the most careful examination of such cases, never found the slightest trace of inflammation in the small veins of the intestine, while no direct proof has been advanced of the mediation of the portal blood in the process.

"I believe we must give the preference to that view which regards the two diseased processes, dysentery and abscess of the liver, as without mutual relation, but as running their course together, dependent upon one and the same cause; in favour of which view is the circumstance, that in hot climates abscess of the liver also very frequently occurs associated with remittent fevers, or consecutive to them, without dissection exhibiting any ulceration of the mucous membrane of the intestine." (p. 175.)

When speaking of the different directions in which the abscess may burst, he directs attention to the fact that, after passing through the diaphragm, it may, in place of perforating the pleura, gradually separate it from its attachment to the parietes of the chest, and extend in this way even as far as the axilla, an occurrence which may give rise, on the opening of the abscess externally, to the belief of the collection having taken place within the sac of the pleura. On the bursting of the abscess

of the liver, we must not immediately look for a striking diminution or disappearance of the tumour, for the reason already assigned, namely, the dependence of much of the enlargement upon the associated hyperæmia and exudation. In abscesses with thick cartilaginous walls, the cavity, after bursting, or artificial evacuation, cannot contract so as to produce complete closure, and, consequently, the patient may be destroyed by the exhaustion resulting from the prolonged suppuration. On this ground, Haspel recommends that the abscess should be opened artificially before the induration of its circumference occurs, and as soon as we are convinced of the presence of pus, not waiting for extensive fluctuation to become apparent. The advice of Dr. Henoch, however, on account of the possibility of an erroneous diagnosis, or of an unusual position of organs, is to defer operating until a surrounding œdema and slight redness of the integument indicate adhesion of the abdominal wall with the surface of the abscess; where adhesion is absent, the operation may give rise to a fatal effusion of pus into the abdominal cavity.

An excellent account of *suppurative inflammation of the portal vein* is appended, of which we regret that our space will not permit an extended notice. Several instances of this affection have been observed, and in one instance it was made the subject of a correct diagnosis by Schönlein. It may arise primarily as the result of direct wounding of the vessel, a well-known instance of which is related by Lambron, where the injury was inflicted by a fish bone, or from inflammation of neighbouring structures, or of parts in immediate relation with the vein, especially abscess in the parenchyma of the liver. When occurring secondarily, it is preceded by the coagulation of the blood in the venous canal, in consequence of the admixture with it of some deleterious matters, such as products of inflammation.

Cancer of the Liver.—In order to explain the origin of the exudation from which cancer proceeds, Dr. Henoch considers the previous existence of a hyperæmia to be necessary. In this again we cannot agree with him, since, although an unusual deposition of plasma, in consequence of hyperæmia, must be admitted as aiding the rapid formation of a cancerous tumour, there is still every reason to believe that the plasma of ordinary nutrition may develop itself into cancer. Proceeding on his assumption, Dr. Henoch regards the admitted frequency of cancer of the liver as due to the readiness with which this organ becomes the subject of hyperæmia. In the early stages of cancer of the liver, nodulation of the hepatic tumour is not to be expected; the only symptoms present being those of hyperæmia or of the early stage of chronic hepatitis, which may continue for a year or longer before any unevenness of surface is exhibited by the tumour. Indeed, there may be no enlargement of the organ at all ascertainable by the touch or by percussion, or the liver may even be smaller than usual, notwithstanding that the cancerous matter is abundant, in consequence of atrophy of the unaffected parts, from their natural supply of blood being cut off by the pressure of the cancerous masses upon some of the portal branches, or by the latter being obstructed by cancerous matter within them. But sometimes atrophy of this kind is compensated by great increase in the cancerous tumour.

Hardness of a cancerous mass, as felt during life, is no evidence of its being scirrhous. Cancerous tumours of the liver, though mostly fixed,

may occasionally be moveable to a trifling extent; this occurs when, as in a case related by Dr. Henoch, the tumour is seated upon a very narrow base; and an instance has been recorded by Boismont, in which cancer affecting the lobulus Spigelii gave rise to a moveable tumour, which was consequently mistaken for a tumour of the pylorus.

Cancerous tumour of the liver often visibly expands the region which it occupies. The tenderness on pressure is more remarkable when the growths are near the surface than when deeply seated in the tissue; but even a tumour superficially tuberous and knotty may be wanting in this respect. A case is related, in illustration, of a man thirty-four years of age, in whom the tumour, felt immediately behind the abdominal wall, was not only free from spontaneous pain and tenderness, but also imparted to the hand a sense of crepitation, and to the ear the "new-leather sound," when the stethoscope was pressed upon it with an alternately greater and less degree of force. Probably, in this case, there was also some partial peritonitis, a supposition only weakened by an observation by Dr. Budd, who once perceived, on slight pressure over a cancerous tumour in the epigastrium, a distinct creaking, like that of new leather, which continued to be felt, more or less obviously, up to the time of the patient's death: on post-mortem examination no traces of inflammation of the capsule of the liver were discoverable, but the creaking could be produced on pressing the exposed tumour. An important diagnostic feature of cancerous tumour is the great rapidity of its growth, which is unequalled in any other disease of the liver. The spontaneous pain usually present may be wanting, especially when the cancer is deeply seated in the organ; but pain may, as in abscess, be present in other parts, such as the extremities, chest, loins, and lower part of the back. The "lancinating pain" is, in Dr. Henoch's experience, more frequently absent than present. Jaundice is not a constant symptom; and; when it does occur, it depends on compression of the ducts external to the liver, or within it, by the cancerous matter, or on cancerous disease of the ducts themselves, retaining the bile within the unaffected portions of the organ. Ascites, if present, may be due either to chronic inflammation frequently associated with cancer of the peritoneum, or to compression of the portal vein, or obstruction of its trunk or branches within the liver by cancerous matter or coagula.

Since cancer of the liver is more common as a secondary than as a primary disease, much assistance may be afforded to our diagnosis by ascertaining the existence of cancer in other organs, an aid which is especially valuable where the more marked palpable signs are deficient, or are obscured by a high degree of ascites. Dr. Henoch refuses Budd's explanation of the secondary development of cancer within the liver, just as in the case of abscess of that organ, on the ground of its being unsupported by direct observation. He assumes, however, the existence of a "cancerous diathesis" as essential to explain the origin of cancer, particularly when it follows upon the receipt of various mechanical and chemical injuries, instances of which are not at all deficient in medical literature.

The question of *curability of cancer* of the liver involves the doctrine of spontaneous retrogression of cancer as maintained by Bochdalek, a change which he states to be most frequent in tuberiform cancer, and in the liver more than in other organs. It is accompanied by fatty degeneration and

shrivelling of the cancer-cells, and, when complete, is indicated on the surface of the liver by a cicatrix-like depression, on which may be mostly distinguished the pale, warty remains of the destroyed cancer. This sort of cicatrization, however, can only be regarded as curative when the peculiar diathesis leading to the further growth of cancer ceases to operate, either spontaneously or under the influence of remedies. Dr. Henoch seems to consider the possibility of this established by two cases of apparent cure of cancer of the liver related by Oppolzer, and one observed by himself, in which, however, no post-mortem examination was made.

Hydatid Disease of the Liver.—Of all the diseases of the liver, with the exception of fatty degeneration, none arises and pursues its course with so little general or local disturbance as this. It is distinguished by the signs of enlargement, the characters of the tumour, and the absence of occasional symptoms, such as pain, which cancer is apt to occasion. Even the enlargement may not be palpable below the margin of the ribs, the right cavity of the chest alone being encroached upon, and under these circumstances the symptoms that arise may be referable rather to the thoracic than the abdominal organs. The tumour, when palpable through the soft parietes of the abdomen, mostly exhibits the smooth semi-globular form of the hydatid sac, in consequence of its projection more or less above the surface of the liver. Sometimes this projection may be as hard as a cancerous tumour, from having been the seat of a calcareous deposition; but for the most part there is a peculiar tense elasticity, observed on pressing it with the finger, which is very distinctive, and in Dr. Henoch's opinion and our own, is more generally applicable to the purposes of diagnosis than the hydatic fremitus of Piorry. Dr. Henoch's assertion respecting the great rarity of the last-mentioned sign is confirmed by our own experience; its existence, however, is not to be denied, nor yet its value when it is perceived.

> "But even in those cases, in which these vibrations are palpable, and furnish a valuable diagnostic criterion, I have not been able to feel them on *every* examination, even when I took the greatest pains to discover them; rather, out of about ten experiments which I instituted, this sign only occurred at the most two or three times quite accidentally." (p. 244.)

Hydatid disease often undergoes a natural process of cure, by the secretion of a plastery matter from the interior of the sac, which exhibits, under the microscope, not only particles of fat and plates of cholesterine, but also the characteristic hooks or other remains of the dead echinococci. In other instances suppuration occurs, which may so completely destroy the hydatid, that only the discovery of the indestructible hooks can demonstrate the origin of the disease. This abscess may burst just in the same way as ordinary abscess of the liver, and the subsequent prognosis similarly depends upon the degree of density of the surrounding wall of the abscess, and the readiness with which it collapses.

The origin of the disease in the introduction of one or more germs into the organism is not to be questioned; but Dr. Henoch again hesitates to admit Dr. Budd's well-known hypothesis, which accounts for the simultaneous presence of hydatids in the lungs, spleen, and mesentery, believing that the number of observations on which it is advanced, is too small to support it satisfactorily. For our own part, we are more disposed to admit Dr. Budd's view in the case of the secondary formation of hydatids

in the lungs, than in the instance of their formation in the mesentery and spleen; since, as Dr. Budd himself very fairly points out, the germs would have to pass backwards from the liver to these parts, in a direction opposed to the current of the portal circulation. If, then, the formation of hydatids in the latter organs is to be attributed at all to germs derived from the liver, these may be imagined, with greater probability, to pass through the lungs and general circulation, and to become arrested and developed in the spleen or mesentery from causes similar to those which first caused their development in the liver, another organ connected with the portal system of veins.

Dr. Henoch only refers at length to two points in the treatment of hydatid disease. One of these is the fomentation of the hepatic region with a saturated solution of salt, in proof of the efficacy of which he has failed to discover one single authentic case; and the other is the artificial opening of the tumour. The dangers of the operation are similar to those which have been adverted to in the artificial opening of chronic abscess of the liver; but though at all times hazardous, Dr. Henoch believes it may be undertaken when the bursting of the tumour appears imminent, whether the cyst be suppurated or not.

A chapter at the end of the volume is devoted to the consideration of *jaundice*, a symptom common to most of the diseases referred to above. When discussing the cause of this symptom, he takes the opportunity of refuting the opinion of those who maintain an origin of jaundice in suppression of secretion. He does this chiefly upon the ground that the biliary constituents are not discoverable in the blood, even the portal blood of healthy individuals, nor yet, according to the results of Müller and Kunde's experiments, even after extirpation of the liver. On the other hand, he maintains that, in all cases. the bile being formed by the cells of the liver (a point, by the way, anything but satisfactorily established), jaundice arises from its reabsorption, as shown many years ago by Saunders, both by the veins and lymphatics of the liver. Still, cases have been sometimes met with, in which marked jaundice has been observed, yet after death the liver and biliary appendages have presented no appearances capable of explaining the occurrence. Dr. Henoch prefers leaving these unexplained to attempting their explanation by the assistance of the undemonstrated assumption of an arrest in secretive process in the liver. The amount of discolouration of the stools indicates the degree in which the passage of the bile into the duodenum is interfered with, an interference which may arise from causes within the liver or its ducts, or external to the latter. In explaining the bilious diarrhœa which sometimes accompanies jaundice, he quotes with approbation the following remarks by Henle.

"I regard even the existence of this disease (polycholia) as doubtful, and believe that, in cases where, besides jaundice, bilious vomiting and bilious diarrhœa occur, the origin of the symptoms is a temporary or incomplete retention of bile. Suppression of the excretion of bile for two or three days is amply sufficient to colour the skin yellow. When arrest of excretion lasts for this period, and then the accumulated bile flows into the intestine, it may appear as if bile were everywhere present in excess." (p. 271.)

Dr. Henoch does not deny the possibility of the occasional origin of

jaundice in spasm of the choledic duct, but calls attention to the length of time that this spasm must last in order to produce it, and to the fact that the prolonged spasmodic closure of the duct around a calculus whilst passing along it, is frequently unproductive of this symptom. He believes that the necessary duration of the spasm, and consequent retention of bile, can only occur where there is some immediate source of irritation, such as a calculus or worm in the duct itself.

The most frequent of all causes obstructing the flow of bile into the intestine, is catarrhal inflammation of the mucous membrane of the duodenum and bile ducts, which Henoch recommends to be treated in the mode which is customary in this country, believing, also, that moderate diarrhœa does not offer any contra-indication to the use of purgatives. He also relates three cases, to show the benefit which may arise from the use of nitro-hydrochloric acid, administered internally and in baths, where the ordinary method of treatment has failed.

We must pass over Dr. Henoch's remarks upon yellow fever and icterus neonatorum, in order to notice his views upon the subject of that form of jaundice which becomes fatal *with cerebral symptoms*, an apparent analogue to which is presented to us in some cases of Bright's disease of the kidneys. At first the jaundice may appear to be of trifling importance, the symptoms being those of ordinary duodenal catarrh; but, after some days or weeks have elapsed, cerebral symptoms, headache, giddiness, somnolence, and typhous phenomena present themselves, sometimes accompanied by convulsions, and the patient dies comatose. This form of the disease may set in with active fever, and blood may be vomited or passed by the bowels. In cases of this kind, with few exceptions, the liver has been found morbidly altered, and presenting the characters, described by Rokitanski, of the "acute yellow atrophy" of that organ. A striking microscopical character of this is a diminution and destruction of the cells of the liver by fatty metamorphosis such as has already been stated to occur as the result of retention of bile within the liver. The bilious colour of the vomited matters and stools frequently noticed, forbids our assuming any obstruction to the flow of the bile into the intestine; and hence, to account for the yellow colour, Dr. Henoch is driven to the admission of "an immoderate secretion of bile, as a result of which, all the bile-ducts up to their fine divisions become excessively full, and the bloodvessels compressed, and consequently, the nutrition of the cells of the liver becomes injured in such a degree as to bring about their destruction by fatty metamorphosis." (p. 291.)

We are still at a loss to see how on this view the bile is prevented flowing from the liver by the ducts, and Dr. Henoch admits his entire ignorance of the origin of the assumed polycholia.

In explaining the mode in which the cerebral phenomena are brought about, he rejects, as unsupported by evidence, the hypothesis which attributes them to the presence of biliary matters in the blood. He regards that which explains them by the presence of hyperæmia, to be supported by the fact, that in many cases, medicines deriving powerfully to the intestinal canal may avert the comatose condition, or, if it be already established, may remove it, and thus preserve the life of the patient. If it be assumed that there is no connexion by way of cause and effect

between the two sets of symptoms, they must, at all events, be admitted as due to the same cause, whether this be a peculiar virus, or, as some have supposed, a primary powerful depression of the nervous system, such as results from violent emotions of the mind.

It has been stated that this union of jaundice with cerebral phenomena is mostly found after death to have been connected with "acute yellow atrophy of the liver." Sometimes, however, this is not discovered; while, on the other hand, cases have been recorded in which this diseased appearance, or one closely allied to it, has been found without any cerebral symptoms having preceded the death of the individual. A case of this kind is related, which only the extent to which this review of the volume has extended prevents us from transcribing.

The fact that we have presented our readers with a lengthened analysis of Dr. Henoch's first volume, proclaims, more than would direct eulogy, our opinion of its value; and the satisfaction with which we have perused and reviewed it, leads us to anticipate a repetition of an agreeable task, when the future volumes come under our notice.

Edward Ballard.

REVIEW XIII.

Beobachtungen über die Korperwärme in Chronischen Fieberhaften Krankheiten. Von Dr. PAUL ALEX. JOCHMANN.—*Berlin*, 1853. pp. 93.

Observations on the Temperature of the Body in Chronic Febrile Diseases. By Dr. JOCHMANN.

IN a former number of this journal* we gave a full account of the interesting researches of Dr. Traube, on "Crises and Critical Days" in acute febrile cases, as judged of by the temperature of the body. The work before us, written by a pupil of Dr. Traube, is based on thermometrical observations made several times daily, on twelve patients suffering from various chronic febrile diseases—viz., 1. Chronic pulmonary tuberculosis; 2. Chronic pulmonary induration; 3. Pulmonary tuberculosis; 4. Double pleurisy, with pulmonary tuberculosis; 5, 6, 7, 8, 9, 10. Pulmonary tuberculosis; 11. Chronic pleuro-pneumonia, with formation of cavity; 12. Chronic pleuro-pneumonia, with commencing Bright's disease.

In the first part of the book the cases are related at some length, and tables are given of the temperature compared with the other symptoms of the case.

In the second part of the book the deductions are given, and these we shall now briefly enumerate.

In these twelve chronic febrile cases the variations of the temperature of the body were very great in different cases.

Daily Variations in the Temperature of the Body.

First Type.—The morning and evening temperatures were entirely, or almost entirely, within the normal limits: the morning temperature being a little less than the evening. This occurred in two cases, Nos. 1 and 11.

* No. 21, January, 1853.

Second Type.—The morning temperature was normal, or below the normal; the evening temperature above the normal. Several varieties of this type can be made.

(*a.*) The rise of temperature in the evening was only slight.

(*b.*) The rise was considerable. In this case the fever had a quotidian or a tertian course—i. e., the great rise occurred every evening or every other evening. It is, of course, to be understood that these were not malarious cases.

Third Type.—The temperature, both morning and evening, was above the normal. Several varieties of this type can be made.

(*a.*) The evening temperature was the highest.

(*b.*) The morning temperature was the highest.

(*c.*) The midday temperature was higher than either morning or evening.

The disease seldom ran its course with the same type; on the contrary, the type was frequently altered, either from exacerbations, from accessory diseases, or from causes which remained totally obscure. The types 3 (*c.*), 3 (*a.*), and 2 (*a.*), were the most constant; 3 (*b.*) and 2 (*b.*) the least so.

If at any time there was an unusual height of temperature at one particular observation, at the next observation the temperature was very frequently unusually low. The reverse did not hold good; a low temperature was not followed by a high one. When the temperature had remained very high for some considerable time, a sudden sinking of it sometimes occurred, and was attended with crises or pseudo-crises—such as purging, sweating, &c., as in acute diseases. Sometimes, without previous exacerbation, the temperature sank very low.

The conditions which produced these various types, could not be ascertained, as the number of cases was so few. Some conjectures can however be made. The first type occurred in cases of early tuberculosis without softening, and of chronic non-progressing pneumonia; it occurred also in cases in which the febrile heat had been reduced by digitalis.

A high morning temperature occurred in those cases where, besides an original chronic disease (tuberculosis, for example), some other cause, such as pleurisy, or purulent formations in the lung were present.

A high evening temperature occurred during increase of the febrile action.

A moderately-high evening temperature occurs from the same cause, but also from return of the bodily strength after the fever has lessened.

After illustrating those facts by reference to the cases, Dr. Jochmann passes on to the interesting subject of

The Relation between the Temperature of the Body and the Sensation of the Patient.

It is well known that in acute febrile cases, the actual temperature of the body, as indicated by the thermometer, stands in no constant relation with the feelings of the patient. The patient may think he is extremely hot or cold when the thermometrical indication would lead to an entirely opposite conclusion. The same rule prevails in chronic febrile cases. It would appear that the sensation of heat or cold perceived by the patient

is chiefly dependent on the amount of blood in the skin: when there is hyperæmia of the surface, the patient feels warm; when there is anæmia, he feels cold. It is now well known that in the stage of shivering, when the patient feels excessively cold, and when, in fact, the skin, being for the time anæmic, may be really cold, the temperature of the blood—i. e., of the rest of the body—is heightened. Von Baerensprung pointed this out in ague, and it has been often witnessed in other cases. We may illustrate some of these facts by reference to one of Jochmann's cases. A patient had a temperature (indicated by the thermometer) of 99·⅕° Fah.; he felt a sensation of unbearable heat; on the following evening the temperature was 104° Fah., and the patient was shaking with cold. A short time before the first observation, the temperature had been 95·⅕° Fah., and the patient had felt neither hot nor cold.

Dr. Jochmann found that if, between two observations there had happened to have been shivering, the temperature in the last observation was invariably increased. If the thermometer was used during the rigor, the temperature was always increased. The following table, in the case of a patient with pulmonary abscess, shows the amount of increase. The thermometer had been in the mouth about fifteen minutes, when shivering commenced, at 25 minutes past 5 o'clock.

Time.	Temperature.	Remarks.
5·25	100·4° Fah.	Shivering.
5·30	100·59 „	„
5·35	101·12 „	„
5·40	101·66 „	„
5·45	102·56 „	„
5·50	103·23 „	„
6·45	....	Cessation of shivering.*
7·30	105·44 „	Commencing feeling of heat.

During shivering fits, however, if the axilla be the part tested, the thermometer attains its maximum much more slowly than in the stage of heat, as the amount of blood in the skin is so much less.

Although shivering is thus attended by rise of temperature, rise of temperature, especially if gradual, does not necessarily imply shivering. Dr. Jochmann believes, however, that an abnormal quick rise of temperature of the body is the principal, perhaps the only, cause of shivering. The feeling of shivering is also very dependent on individuality and custom.

As shivering and, sometimes, subjective feeling of cold are thus coincident with rise of temperature, so subjective feelings of heat are often coincident with sinking temperature.

Relation of Changes of Temperature to Sweating.

From observations on phthisical patients, Dr. Jochmann believes that in these cases at least two varieties of perspiration can be distinguished.

1. The symptomatic sweats, viz., those which, when well marked, return every night, and the severity of which is conformable to the severity of the disease. During these sweats there is not only no sink-

* The patient being restless, the thermometer was removed.

ing of the thermometer (except from other accidental circumstances), but, on the contrary, the heat of the body is increased. Such sweating does not, therefore, at all moderate the fever, but only exhausts still more the strength of the patient.

2. Critical sweats, which come on from time to time in phthisis, and are attended with rapid sinking of temperature; such sweats may be replaced by diarrhœa. They come on indifferently by day as well as by night, and are analogous to the critical sweats of acute diseases.

Relation of the Changes of Temperature to Changes in the Frequency of the Pulse.

The pulse varies in chronic febrile cases, from so many circumstances, that a very imperfect opinion can be derived from it, as to the amount of fever and of temperature. Sometimes, however, the pulse will point out the existence of a fever not indicated by the temperature. This occurs only in cases of very slight intensity, when the slight excess of heat which would be caused by the fever is balanced by the loss of heat always produced by the withdrawal of food (inanition). The pulse is usually high when the temperature is high, but the relation between the two is very variable. The frequency of the pulse is evidently not dependent on the temperature of the body; it is influenced, the author thinks, by the condition of the bodily strength, being quicker as this becomes exhausted; by the movements and efforts of the patient, and by the time of observation. The pulse is generally quickened in the evening, and this the author ascribes in part to the food taken during the day.

In the last few pages of his book the author refers to the influence of some medicines on the temperature of the body in chronic febrile cases. Quinine was found in a single case, in which the fever had a tertian type, to remove this type and to reduce the temperature. It was employed in another case, but the observations are imperfect. Digitalis produced a marked lowering of the temperature, as in the experiments of Heise. The evening fever was especially affected; while the morning temperature was not influenced at all, or but little so. This effect was evident for a long time after the remedy was left off. Dr. Jochmann believes digitalis to be indicated in those cases in which the morning temperature is normal, and the evening temperature is high, and in which the bodily powers are not too much weakened. Cod liver oil was used in several cases in which the low morning temperature indicated inanition, and in which the evening temperature was not very high. Its influence on the temperature is not mentioned, but it appears to be very useful in those cases in which the morning temperature sinks very low, from inanition, while the evening temperature is very high from hectic.

PART SECOND.

Bibliographical Record.

ART. I.—*The Life of Girolamo Cardano, of Milan, Physician.* By HENRY MORLEY, Author of 'Palissy, the Potter.' Vols. I. and II.—*London*, 1854.

IN these two volumes, Mr. Morley has selected one of those early physicians whose career is summed up in half a page in the histories of medicine, for a most elaborate and critical biography. He has not had, we suspect, quite so congenial a theme as in the case of the simple, earnest, and truth-inspired potter: but yet the mingled singularity, superstition, and subtlety of Jerome Cardan, the celebrated physician of the sixteenth century, appear to have had some considerable attraction for him. We never remember to have read a book in which the results of no little toil and antiquarian research are communicated with so little effort, and in which the writer has performed his task in a more genial and merciful spirit.

Before we read these volumes, our knowledge of Jerome Cardan was small enough. That he was one of the early protesters against some of the Galenic doctrines (especially the dogma "contraria contrariis"); that in a brief mention by Haller, he is characterized as "vir mirifici ingenii, sed instabilis et irrequietus, bis de catalogo patriorum medicorum rejectus;" and that, after spending the early part of his life in the bitterest poverty at Milan and Sacco, he ended it after various vicissitudes in a state of insanity at Rome, comprise the facts usually recorded of a man who was celebrated throughout Europe, who had written four hundred and twelve works, who was sent for from Italy to Scotland, to cure the Archbishop of St. Andrew's, the Regent Hamilton's brother, who was consulted in the case of Edward VI., and for whose advice frantic admirers contended almost at sword's point.

So unconsidered and unthought of, Jerome Cardan had probably remained, had not the genius of antiquarianism shaken the cobwebs from his multitudinous works, and with a perseverance worthy of a better theme, laid bare before the eyes of this inquisitive age, the physician of the sixteenth century, half man of sense, half man of fancy, astrologer and physician, casting horoscopes, collecting simples, diagnosing diseases by symptoms, and judging of their issue by the stars, discoursing as to the omen of a black cat mewing, and yet, at the same time, having looked deep enough into the secrets of the human frame to be able to suggest, before Harvey had discovered the circulation, the experiment of the transfusion of blood. Thanks to Mr. Morley, we are now familiarly acquainted with

this singular man, who born in ignominy, educated in contempt, and matured in poverty, was destined to shine forth at last the all-considered genius of the time.

We do not blame Mr. Morley for having kept a good deal away from medical doctrines. Writing for the public, he has attempted a history of actions, rather than one of opinions. Here and there, however, he has given some curious details of the medical doctrines of the time. The opinion given by Cardan of the nature of the disease which Hamilton, Archbishop of St. Andrew's, laboured under, is very interesting.

"Cardan had learnt, in addition to the facts mentioned by Cassanate in his letter, one or two particulars. These were, that the archbishop's periodical attacks did not agree always, but only generally with the changes of the moon; that sometimes when he took care of himself, he might get through fifteen or twenty days without them. That the duration of each attack seldom exceeded twenty-four hours, and that it sometimes remained upon him twice as long. That his Grace slept well, but that on account of the urgency of his affairs, he never took the quantity of sleep requisite to free himself from crudities, especially since he was a great eater and drinker. That he was irascible enough, had a skin that exhaled freely, a chest of fair size, and rather a thin neck.

"Upon the case, after he had personally studied it, Cardan's opinion resembles a long clinical lecture. It is a very acutely-reasoned study of asthma, based upon principles laid down by Galen. Wonderfully absurd seems now its medical philosophy; but in the year 2154, what will be said even of our physic? Let us be modest in our treatment of the physic of Cardan. He did not believe with Cassanate, that the matter finally expectorated had remained in his Grace's brain, as it collected there during the intervals between the attacks. If so, he thought that the operation of the intellect must be impeded, and that the lord archbishop would not have, as he had, the red complexion of a healthy man; moreover, the matter so collecting, and long standing in the head, would turn corrupt. He believed that the thin fluid discharged was partly serous humour, partly condensed vapour, which descended from the brain into the lungs, not through the cavity of the windpipe, for if so, it would be coughed out during its downward passage, but through its coats, as water soaks through linen. This thin humour and vapour he supposed to be originally drawn into the brain by the increased rarity in the substance of that organ, caused by undue heat. Heat makes all things rare, and rarefaction in one part of the body, to express the idea roughly, produces suction from another. The thick expectorated matter was formed, Cardan thought, from the food." (Vol. II. pp. 113—14.)

We have not quoted this to illustrate the kind of matter in this work, for it would not do it justice: nor to show what kind of solemn nonsense wise men can talk: but because we observe in it that Cardan adheres to the Galenic doctrine of the flow of mucus from the brain to the lungs, a tenet which we imagined he had strongly combated.

In spite of his theories, Cardan's practice was good; he ordered a capital system of hygienics, including proper diet, frictions, the shower-bath, and other means; and in no long time cured the archbishop, to his own great honour and profit.

Our space will not allow us to dwell longer on this entertaining book. We will only observe, that Mr. Morley's copious antiquarian knowledge is [illegible] by the rapid, but happy, sketches which he gives of every person of importance whose name is brought forward by the incidents of Cardan's diversified career. The treatment of Cardan himself is very pleasantly done; the biographer has selected the most merciful, and no

doubt the truest, side from which to view his subject; and we can only hope that if, in the years to come, the great physicians of our day are to be raked up from amidst the *débris* of a past generation, they may meet with treatment equally kind, charitable, and sagacious, as that which has now fallen to the lot of the physician of Milan.

Art. II.—*Medicines, their Uses and Mode of Administration; including a complete Conspectus of the three British Pharmacopœias, &c.* By J. Moore Neligan, M.D. Edin., M.R.I.A., &c. Fourth Edition.—*Dublin*, 1854.

When a medical book has reached a fourth edition, it may fairly be considered beyond the pale of criticism. Its reputation has been made, and a reviewer need generally do little more than announce a fresh issue, and repeat the favourable judgment formerly passed. Dr. Neligan's work may, however, fairly claim something more than this: it has been very considerably improved; its utility has been increased by the plan of giving at length the formulæ of the three British Pharmacopœias; and the latest researches have been embodied. The work has not, however, been made unwieldy by these augmentations; it still remains concise and practical. A single quotation will illustrate this.

After describing the mode of preparation of the ferruginous medicine lately come into vogue—the "Pulvis ferri," (viz., metallic iron reduced to a fine powder by means of hydrogen)—Dr. Neligan continues:

"*Adulterations.*—Since the introduction of the *pulvis ferri* into practice, the demand for it has steadily increased, and consequently, its preparation being difficult, troublesome, and expensive, it could scarcely be expected to escape adulteration; it is, however, rather a sophistication than an adulteration which has been practised with respect to this preparation. The fraud, which has recently attracted much notice, in consequence of a dispute to which it has given rise between two rival wholesale chemists in London, consists in the substitution of the magnetic black oxide of iron for the powder of iron. The taste is at once sufficient to detect this, the latter being perfectly tasteless when placed on the tongue, while the former has the peculiar inky taste of the ferruginous preparations. Chemically, they may be distinguished by the powder of iron being completely soluble in dilute sulphuric acid with copious effervescence, while the magnetic oxide effervesces not at all, or but slightly, owing to the presence of some sulphuret of iron: the former solution also gives a green precipitate; the latter a black one, with an alkali.

"*Therapeutical Effects.*—Iron, like other metals, does not exert any influence on the human system while it retains the metallic state; but as it is very readily oxidated and converted into salts, this change takes place in the stomach soon after it is swallowed, and then the effects of a tonic are produced. *Iron filings* were at one time much used in medicine, but in the present day they are scarcely ever employed in regular practice; the dose of them was from ten grains to half a drachm, administered in the form of electuary or bolus made with treacle or honey.

"More recently, the employment of metallic iron, reduced to a state of minute division by means of hydrogen, as in the above formula of the Dublin Pharmacopœia, (*fer réduit*, of the French), has been employed on the continent, its use having been first introduced by MM. Quevenne and Miquelard. The chief circumstance to be attended to during the operation of preparing it is the state of the

temperature. If it be not sufficiently high, the reduction does not take place; and if it be too high, the iron is reduced, but is agglutinated into ductile plates. When well prepared it is in the form of a fine light powder, of a bright greyish slate colour, occasionally darker, in very minute division, and free from any trace of sulphur. The advantages which this preparation possesses are, first, that it is readily acted on by the weak acids—the lactic and muriatic, which are ordinarily present in the gastric juice during digestion; and secondly, that it is free from the inky taste, which the preparations of iron possess in a degree proportioned to their solubility; a property rendering it peculiarly applicable for children. I have used the *pulvis ferri* very extensively since the last edition of this book was published, and with the best results; indeed I consider it superior in most cases to any other ferruginous preparation, being especially adapted for persons in whom the digestive organs are in a feeble or debilitated state, as is so frequently the case when indications exist for the administration of iron. The dose is from one to ten grains; it may be given in powder, pill, or bolus. . . . .

"It has been of late proposed in France to administer manganese in combination with iron, from a fancied notion that it would be thus rendered more readily assimilable by the system, a notion, in my opinion, resting on no good foundation. Nevertheless, the compounds of iron and manganese have just at present acquired a sort of fashion, and various formulæ have been proposed for preparations containing them; of these probably the best is that by Dr. Speer, of Cheltenham, for a *saccharated carbonate of iron and manganese* prepared as follows:—'Finely powdered sulphate of iron, ℥iiij. ʒj.; carbonate of soda, ℥v.; sulphate of manganese, ℥j. ℈j.: white sugar, ℥iss.; dissolve each of the three first-mentioned ingredients in a pint and a half of water, add the solutions, and mix them well; collect the precipitate on a cloth, filter, and immediately wash it with cold water; squeeze out as much of the water as possible, and, without delay, triturate the pulp with the sugar, previously reduced to a fine powder. Dry it at a temperature of about 120° Fahr.' . . . . The dose is five grains, gradually increased up to one drachm, three times daily." (pp. 497—99.)

In reading through this work, as in the case of every other treatise on Materia Medica, we are forcibly reminded of the utter absurdity of our present unparalleled system of preparing different medicines for English, Scotch, and Irish patients. How long are our three Pharmacopœias—Cerberus-like, one body and three heads and tongues—to continue disjoint, in these days of rapid travelling? Weights, remedies, formulæ, all different; and yet the diseases are the same, and the patients are fellow-subjects! Are we to wait till Medical Reform is agreed upon by the representatives of our various institutions, even for so simple an alteration as a single national Pharmacopœia? If so, we must make up our minds to endure our three-headed progeny for many years to come,—till men grow reasonable, and think more of the rights due to their profession and to the public, than to their imperfect corporations. Till that good time shall come (and come it must), we must learn from works like the one before us what alterations we must make in our prescriptions, according to the residence of our patients.

ART. III.—*The Irish School of Medicine as it is, and as it ought to be. An Address introductory to a Course on Pathological Anatomy and Histology.* By THOMAS S. HOLLAND, M.D.—*Cork*, 1853. pp. 19.

DR. HOLLAND's object in writing this pamphlet is shown in the following quotations:

"That the period has arrived when scientific centralization should cease, is proved by the establishment of scientific instruction in the provinces: further, that the Intelligence of the country feels we are entering on a provincial era, is clear from the success of the new institutions; and this lecture cannot find a better, a higher, or more appropriate subject, than the consideration as to how a medical school, destined to raise the character of Irish medicine by imparting to it a rational, inductive, and truly scientific spirit, such as constitutes the leading feature in modern physic, can be formed in this city. . . . .

"The Dublin School of Medicine owes the celebrity it has obtained to the pre-eminently successful manner in which it has carried out the system of clinical instruction derived from the Germans; but, unfortunately, the cotemporaries of the founders of the Irish School did not also imitate or acquire the highly scientific and truth searching character of the German mind; had they done so, they would never have allowed the reputation of the school to be limited to, or dependent upon, the cultivation of any one branch of medical science. Though our teachers have most accurately and successfully studied the diagnosis of the diseases of the thoracic viscera, their treatment, and that of fever, yet it must be evident, to all who are acquainted with Irish medical literature, that the affections of the abdominal viscera and nervous system have not been as carefully observed as in other schools.

"The high character of Irish medicine was for some time so evident that it became almost proverbial, and formed the subject of all introductory lectures, the boast of the senior to the junior student, but no one has hitherto ventured publicly to inquire whether structural anatomy, physiology, pathological anatomy, or organic chemistry, existed as a part of our system of medical instruction.

"Further, the belief in the immortal fame of the Irish School became so implicit and universal, that students, many of whom are now Doctors, acted on the principle, that by learning what their teachers had discovered, and reading the three or four Irish medical classics, the culminating point of medical knowledge was attainable.

"I cannot, on this occasion, forbear publicly expressing an opinion which I have repeatedly stated in private—that the Irish School of Medicine has indeed reached its culminating point, and must of necessity decline, if we continue basking in the sunlight of our teachers' names. How I revere these teachers is known to those best acquainted with me, and how I endeavour to follow their high example is felt within myself, though I cannot resist the conviction that this will not be accomplished by merely traversing the roads they have already cleared. Irish medicine must assume a new character; a truly scientific spirit must be re-instilled into our school, if we desire that it shall keep pace with the advance of science." (pp. 6, 8, 9.)

The author thinks, then, that the Irish teachers are lagging behind the wants of the age, and do not give their students an opportunity of learning how to apply the new truths discovered by the physiologist, the microscopist, or the chemist, to practical medicine.

Unacquainted, as we are, with the details of the system of tuition pursued in the Irish hospitals, and feeling nothing but respect and gratitude towards those great men who have been, or still are, connected with that distinguished school, we can yet believe that Dr. Holland's censure may be well founded. But the censure should have a wider application; the fault complained of is not simply Irish, it is national; the means for teaching histological anatomy and micro-chemistry—for laying a sound basis of experimental physiology—and for applying all these means of investigation to practice—are deficient in every medical school in the kingdom. In some schools, by the zeal of one or two individuals, the deficiency has been to a certain extent supplied; but yet how imperfectly

supplied, let those answer whose earnest endeavours, even after years of labour, seem yet scarcely to have broken the ground under their feet.

To what circumstances we are to attribute our deficiency in some branches of medical education, we can give, as far as England is concerned, a ready answer. The fault does not rest with the schools and teachers: it is caused by an imperfect medical curriculum—by a vicious system, which, like many things English, is a compromise between old habits and novel improvements, in which the old is neither altogether shaken off, nor the new altogether put on. To give the medical student the power of being educated in the way in which he could be, and ought to be, his period of study must be extended; a different system of tuition, more instructive than the routine of formal lectures, must in many cases be adopted; and a rigid system of examination must ascertain, at every step of his progress, whether he is sufficient master of one subject to be allowed to begin the next. But when shall we see such an Utopia? Most men will say it is impracticable and chimerical. Nevertheless, it has been carried out, to a certain extent, in other countries; to an extent, indeed, that leads us to believe that no real difficulty can exist in this country, if men will only agree as to what has to be done, and bring to its accomplishment the ordinary energy of Englishmen. In order to aid this result, and as our contribution to the good work, we propose to give, in some early numbers of this Journal, an account of the system of tuition pursued at several of the great continental schools: then, from their riches, we shall discover our poverty.

Dr. Holland has done good service in bringing this subject forward. He has been evidently actuated by the best motives, and nothing but a strong sense of duty could have led him to blame the great medical school which has reflected so much honour upon Ireland. His remarks should be received in the spirit in which they were written; and it would be well if all of us, and not merely the Irish physicians, would draw from them the lesson they are meant to convey.

ART. IV.—*A History of the British Stalk-Eyed Crustacea.* By THOMAS BELL, F.R.S., F.G.S., F.Z.S., President of the Linnæan Society, Professor of Zoology in King's College, London. Illustrated by 174 Wood Engravings.—*London*, 1853. 8vo, pp. 386.

WE are happy to announce the completion of this valuable addition to Mr. Van Voorst's beautiful series of monographs on the various classes of the British Fauna; and having given our readers some account of Mr. Bell's work* whilst in progress, we need do no more than refer to some of the contents of the parts of it which have subsequently appeared. The most valuable of these is the "introduction," which includes, together with such a general account of the class as no one could write without an intimate and detailed acquaintance with its organization, an admirable and complete summary of the researches of Slabber, Vaughan Thompson, Ducane, H. Goodsir, R. Couch, and others, on the extraordinary metamorphoses which many of these animals undergo. The credit of the

* Vol. ii. p. 219.

first discovery of these metamorphoses—whose existence, though at first questioned by some of the most eminent carcinologists, is now universally admitted—has been usually assigned to Mr. Vaughan Thompson, whose researches were made in the Cove of Cork, in the year 1823; but Mr. Bell has shown, by an extract (with figures) from a small work published by Slabber, a Dutch naturalist, in 1778, that the capital fact of the metamorphosis into a higher form, of those anomalous creatures on which Bosc afterwards founded his genus *Zoea*, had been discovered by him ten years previously; so that, while Mr. Thompson may justly claim the merit of having carried out this doctrine to its full development, it is evident that he derived the first suggestion from Slabber, to whose observations he refers. And it is singular that he should have imputed to his predecessor the very same deficiency in care, which those who objected to *his* conclusions afterwards charged against himself; namely, "that he lost his *Zoea* in changing the sea-water, and that the new form came with the added portion."

There is no class which supplies a more satisfactory example than does that of Crustacea, of easily recognised conformity to a general "archetype," notwithstanding very wide variations in special details; and it is on that account peculiarly worthy of study, by those who aim at making themselves acquainted with the *fundamental plan* of the organized creation. As Mr. Bell remarks:*

"When we consider the almost endless diversity of form under which the species composing this class of animals appear, the astonishing discrepancy which exists in the forms and relative proportions of the different regions of the body, and other parts of their organization, for the performance of offices and functions equally various, and see that all these diversities are produced only by modifications of a typical number of parts, we cannot but be struck by so remarkable and interesting an illustration of the great economical law, as it may be termed, that, *the typical structure of any group being given, the different habits of its component species or minor groups are provided for, not by the creation of new organs or the destruction of others, but by the modification in form, structure, or place, of organs typically belonging to the group.*"

Art. V.—*The West Indies, before and since Slave Emancipation; founded on Notes and Observations collected during a Three Years' Residence.* By John Davy, M.D., F.R.S., &c., Inspector-General of Army Hospitals.—*London*, 1854. 8vo, pp. 551.

It is a sufficient reason for our notice of this work that it proceeds from a distinguished member of our profession, and that it is the product of observations and inquiries carried on during his period of service in the West Indies, in the medical charge of the army. He has endeavoured to give a faithful record of the social state in the Windward and Leeward Islands, as he witnessed it between the years 1845 and 1848; the time at which that alteration of the Sugar Duties was effected, which, next to the Emancipation of the Slaves, had the most important influence in producing that depression of their pecuniary interests under which they are now suffering. Dr. Davy passes each island under review, seriatim;

* Introduction, p. xiii.

describing first its physical geography, geology, scenery, and climate; and then giving an account of the various classes of its population, the state of agriculture, commerce, education, crime,—everything, in fact, that can be considered to have a bearing upon those great social and economic problems which are still open in regard to these interesting colonies. To the accuracy of many of these descriptions we can bear our personal testimony, having ourselves visited many of these islands some years since, and retaining a very vivid recollection of several of their most interesting features. And we have no doubt, from the impartial spirit everywhere displayed by Dr. Davy, that the same accuracy will be found to prevail in the delineations of the social condition of their population, which has greatly changed since we had ourselves the opportunity of observing it in the last year of the system of slavery.

One of Dr. Davy's objects in this publication has been the vindication of the coloured races, which form so large a part of the West Indian population; "having," as he says, "the firm conviction, that the low and degraded state in which they were sunk, and from which they are but slowly emerging, has been owing, not to any inherent inferiority of nature or of mental capacity, but to the dire circumstances of their former condition in the state of slavery." To us the wonder has always been, that since they have obtained their freedom, in a condition of such little preparation for it, they have behaved as well as they have done; and we are satisfied that the difficulties which the planters encounter in inducing them to labour for hire, have arisen at least as much from the notion that labour is a degradation to a free man—a notion which everything in the system of slavery tended to encourage—as from any physical or moral disinclination to labour in itself considered. Opposed as the slaveholders were (as a body) to any measure for the amelioration of the system, and accepting emancipation, not as the removal of an incubus, but as the unjust infliction of an evil, it was not very unnatural, though it was most improvident, that they should do little or nothing to prepare for it; and they are now reaping the bitter harvest of their neglect of those means of *training* the slave population to the habits of free labourers, which prudence dictated as requisite to promote the future welfare of both parties. That this is the case is no merely theoretical conclusion; for it is borne out by the fact, that in Barbadoes, where an agricultural society had been for some time exerting itself for "the improvement of plantership," this society exerted itself, on the abolition of slavery, with the aim of ameliorating the condition of the labourers as *men*—as thinking, responsible, moral beings; and the consequence of the generally enlightened system pursued in this island has been such, that whilst the production of sugar in the time of slavery never reached 30,000 hogsheads, it reached above 40,000 in 1851, and above 50,000 in 1852.

It is another of Dr. Davy's objects, therefore, to hold up the favourable results of the enterprise and prudence displayed by the Barbadian planters, both as an incitement to still further exertions on their own parts, and as a stimulus to the planters of other colonies; and all he asks from the Home Government is a postponement of that final equalization of the Sugar Duties which was to have taken place in the present year, but which has been since suspended in order to help to pay for our contest with Russia.

"It is an ill wind that blows no one any good;" and our West Indian colonists may feel that they owe a debt of gratitude to the Emperor of Russia for thus securing to them a little longer space for preparation. We hope, however, that they will make good use of this piece of good fortune while it remains to them: by taking advantage of all that science and experience dictate as likely to improve their system of sugar production; by the introduction of new objects of culture; and by measures adapted to act beneficially upon the moral and intellectual condition of the coloured population, and to show them that the best interests of all classes are really bound up together.

All who take any interest, whether political, commercial, or philanthropic, in these colonies, must feel greatly obliged to Dr. Davy for having so well employed his opportunities of observation, and having placed before the public such a valuable collection of materials for a right appreciation of their condition and prospects.

Art. VI.—1. *The Microscope, and its application to Clinical Medicine.* By Lionel Beale, M.B. Lond., Professor of Physiology and General and Morbid Anatomy in King's College, London.—*London*, 1854. 8vo. pp. 303.

2. *The Micrographic Dictionary: a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects.* By J. W. Griffith, M.D., F.L.S., and Arthur Henfrey, F.R.S., F.L.S. —*London*. Part I.

Several excellent treatises on the use of the microscope have been published in England and on the continent. Generally they are learned dissertations, going profoundly into the subject, and entering into most copious details. Admirably adapted for those who are well acquainted with the subject, they are, without exception, little fitted for beginners: and, with few exceptions, they have never been projected with a view especially to assist practical physicians in the use of the microscope.

Dr. Beale's work appears to us everything that could be desired to fill the space thus left vacant. His experience as a teacher has enabled him to know exactly what beginners, and especially medical students, require to be taught: and his knowledge of medicine has allowed him to give a practical character to all his illustrations which will render his treatise, we are convinced, extremely popular in the profession. The first part of the work is occupied with a description of the microscope, and of the apparatus required in using it in the examination of animal tissues; then the mode of using it, with details as to mounting preparations, putting up specimens, &c., is related; the latter part of the work is occupied with a brief but excellent account of the microscopic characters of healthy and diseased tissues, of urine, vomit, blood, pus, &c. The vast subject of micro-chemistry is touched upon, and the principal re-agents are passed in review. The chief microscopic objects are illustrated by very excellent cuts and engravings.

In the histological part of the work all the most recent facts have been brought together; the descriptions are clear, yet concise, and give a

complete outline of the whole subject of medical histology. We have no hesitation in recommending most strongly to students this work of Dr. Beale's, as containing exactly the information which learners require. To more experienced microscopists we may also observe, that there are many very practical instructions scattered throughout the work, and that various formulæ are given for preservative liquids, &c., which will be found very useful.

The second work is one of greater scope; as its title implies, it is intended to include in alphabetical order a description of all microscopic objects. The attempt is Herculean: whether it will be successful we can scarcely tell at present, as only the first part has reached us. Certainly this part, as far as we have gone into it, is extremely well done, and if the same fulness and accuracy can be kept up, the 'Micrographic Dictionary' will be most valuable. The references to authorities given at the end of each article are tolerably full; we hope that the authors will endeavour to carry out this feature of the work, so as to give a complete survey of the previous literature on the subject. If they can do this, their work will have a permanent value; if they do not do so, however accurate the microscopic descriptions may be, the rapid progress of observation must of course soon supersede them. We trust also that the authors will keep to their promise of issuing the work regularly every month: and not allow, as in the case of some other serials, the first number to become obsolete, while the last shines with the latest discoveries of science.

ART. VII.—*Manual of Diseases of the Skin.* From the French of Cazenave. By THOMAS H. BURGESS, M.D. Second Edition.—*London,* 1854.

WE lately directed attention to Chausit's 'Treatise on Dermatology,' as containing a good account of Cazenave's opinions and practice. We have here, however, in our native English, Cazenave's own work, well translated and well edited for the second time by Dr. Burgess. Many notes are added by the translator, which supply the deficiencies of the author, and give additional value to the work before us. We recommend it very strongly as an excellent practical work.

ART. VIII.—*The Transactions of the American Medical Association.* (Instituted 1847.) Vol. VI.—*Philadelphia,* 1853. pp. 869.

THE American Medical Association has issued the annual bulky volume, containing, as in former years, many most valuable papers. Besides the reports on the ordinary business of the association, and medical education and medical literature, there are ten original papers. One of these, Dr. Meigs's 'Treatise on Diseases of the Neck of the Uterus,' has already reached this country in a separate form, and we shall review it in an early number. An inquiry into 'Typhoidal' Fevers,' by Mr. Campbell, contains a careful and critical digest of late works on this subject, especially those of Bartlett, Jenner, and Flint. Dr. Waldo Burnett,

whose name is well-known in Europe as that of an indefatigable and conscientious observer, has contributed a most elaborate paper on 'The Physiology, Pathology, and Philosophy of the Cell, to which is added its History and Criticism.' The title is a quaint one, but expresses the scope of the article. The other principal articles are surgical and obstetrical, on the following subjects: 'On Coxalgia,' by Dr. March; 'On Morbid Growths within the Larynx, with a full table of cases,' by Dr. Buck; and 'On the Surgical Treatment of Fibrous Tumours of the Uterus.'

We may observe that a communication has been addressed to us, pointing out that in our notice of the last volume of the Association, we stated that a great part of the work was occupied with reports on the epidemics of New England and New York; besides, however, New England and New York, various other places should have been mentioned. But as we were anxious only to give an idea of what the rest of the volume contained, without occupying space by minutely specifying every place from which reports were sent, we omitted the full details.

ART. IX.—*Cholera Morbus. Guide du Médecin Praticien dans la Connaissance et le Traitement de cette Maladie.* Par le Dr. FABRE, Rédacteur en chef de la 'Gazette des Hôpitaux.'

THE active editor of the 'Gazette des Hôpitaux' has favoured the world with a work of considerable value. The first 150 pages are occupied with a brief sketch of the various epidemics, with a description of cholera, and with a short discussion as to its nature. This part is the least valuable portion; it is sketchy and imperfect; and exhibits the national fault of being almost entirely drawn from French sources, and being a *résumé* of French opinions. The last 200 pages, devoted to the treatment of the disease, are more useful. M. Fabre has given an account, in alphabetical order, of all the remedies employed in France during the epidemics of 1832, 1849, and 1853. This section, 'Dictionnaire de Thérapeutique, appliquée au Cholera Morbus,' gives a good account of the French practice, arranged in a way that renders reference to each remedy easy. No attempt is made to determine the real value of each medicine. An extract or two will show the kind of information to be obtained from this part of the volume.

"*Acid Fluoric.*—M. Magendie has employed this plan in the typhoid period, at the request of Ampère, who had hoped some beneficial results from this powerfully caustic acid, which traverses and instantly corrodes the tissues. In one case he obtained, or *thought he had obtained,* a very favourable result in the case of a woman who, for some days, had been in a typhoid state, which had resisted all other remedies. After having applied on the forearms a layer of this acid, he saw her come to herself, regain consciousness, and lose the lugubrious aspect of typhus. But simultaneously, adds loyally the learned Professor, the supporting treatment was continued, and, I believe, even that rum was given her, on my advice, after the employment of the fluoric acid. On another occasion, M. Magendie has made use of this acid without success." (p. 199.)

"*Croton Oil.*—Croton oil, recommended by the Indian physicians, has succeeded once in the hands of Mr. Bally.

"M. Cauvière, of Marseilles, has given to four patients, a dose of two drops.

Three died the day of their admission, without any evacuation being produced by the remedy. He has repeated the dose in the fourth case of the patient, who lived two days. The stools were numerous, but they were so before the administration of the oil. Probably the remedy was not absorbed." (p. 241.)

These extracts must suffice, and we have only further to remark that, as usual, most of the authorities named are French, and but few are German, English, or American.

Art. X.—*Summary of New Publications.*

In addition to the works reviewed or noticed in previous pages, we have received several others, which we propose shortly to enumerate.

Several works on *Medicine* have reached us from Germany; a 'Medical and Surgical Encyclopædia for Practitioners'* has been commenced, and is to be finished in six volumes; it appears to be a good work. Dr. Henoch has published the second volume of his treatise on 'Abdominal Diseases;'† a review of the first volume appears in our present number. In the second volume, the diseases of the spleen and the stomach are considered at considerable length.

Leubuscher has commenced a work on 'Diseases of the Brain.'‡ Only the general pathology and therapeutics are as yet touched upon; but it is evident that the work, when completed, will be a most valuable one. A fourth edition of Zehetmayer's admirable 'Lehrbuch der Perkussion und Auscultation,' edited by Oppolzer, has been published. It does not call for any remark. A little pamphlet on the 'Development of Air in the Blood'§ is valuable as containing references to all the cases hitherto published. We shall review it at length in an early number.

From France we have received a bulky treatise on 'Diseases of the Skin,' by Devergie,‖ which we reserve for review. A work on 'Medical Electricity'¶ also demands an extended notice. A treatise on 'Medical Chemistry,' guaranteed by the familiar names of Becquerel and Rodier,** will be welcomed in England; though, after reading it, we must confess that we felt some disappointment. It is scarcely what we expected, nor what we should have thought French science would have required. Still there are numerous important facts in it.

From America we have received, in addition to the works previously noticed, a work on 'Malaria and Pneumonia,' by Dr. de la Roche, and a pamphlet on the 'Yellow Fever of New Orleans, in 1853.'

In our own country an important work on 'Epilepsy,' by Dr. Radcliffe, and an extremely interesting treatise on 'Vertigo,' by Dr. Russell Reynolds,

* Medicinisch-chirurgische Encyklopædie für pratische Aertze. In Verbindung mit mehreren Aerzten, herausgegeben von Dr. H. Prosch und Dr. H. Ploss. Leipsig. Ersten Band, Erste Lieferung.

† Klinik der Unterleibs-Krankheiten. Von Eduard Henoch. Zweiter Band. Berlin, 1854.

‡ Die Pathologie und Therapie der Gehirn-Krankheiten. Von Dr. Leubuscher. Erste Abtheilung. Berlin, 1854.

§ Luft im Blut. Von Dr. G. Oless. Stuttgardt, 1854.

‖ Traité pratique des Maladies de la Peau. Par Alph. Devergie. Paris, 1854.

¶ Histoire de l'Electricité Médicale. Par M. J. Guitard. Paris, 1854.

** Traité de Chimie pathologique appliquée à la Médecine pratique. Par M. A. Becquerel et M. A. Rodier. Paris, 1854.

demand future review. Dr. Hamilton Bell has published a work on the 'Circulation and Nervous System in Reference to Disease.' It is apparently intended to explain the action of the tincture of the sesquichloride of iron in erysipelas; but we must confess that we cannot appreciate Dr. Bell's hypotheses and explanations, as much as, out of respect to the author, we could have wished. His facts, however, make out a good case for the treatment of erysipelatous inflammations by iron.

A second edition of Dr. Walshe's excellent treatise on the 'Diseases of the Lungs and Heart' has been issued. So much new matter has been introduced, that we look upon it almost as a new work, and shall, as soon as the demand on our space permit, consider some portions of it at length.

In *Surgery*, the only new work of importance is one by Dr. Yvaren on the 'Métamorphoses of Syphilis.'* Properly speaking, the title should have been 'Concealed or Latent Syphilis.' The work is founded on 125 cases, collected from different authors or from personal observations. In 67 of the cases, the real syphilitic origin of the disease was not at first recognised. In 40 of the whole number of cases, the affection was of the nervous system—viz., cephalalgia, odontalgia, convulsions, epilepsy, insanity, paralysis, &c. In 29 cases, the membranous tissues (mucous membranes, conjunctiva, &c.); and in 42 cases, the parenchymatous organs (lungs, liver, &c.) were the seats of concealed syphilis. In 13 other cases, the syphilitic alterations of organs assumed a completely cancerous character. The diagnoses of these affections is discussed at great length and is founded, partly on the existent symptoms, and partly on the previous history. In four-fifths of the whole number of cases, other unequivocal syphilitic symptoms, such as cutaneous disease, nocturnal pains, caries of bones, &c. were present. In discussing these points, the author alludes to all the most difficult questions connected with syphilis, its origin, its connexion with mercurial cachexia, &c. We have, however, said enough to give a general idea of the work, and shall defer a more complete analysis for the present.

Mr. Maclise continues the publication of his beautiful plates, and Mr. Toynbee has issued a second edition of his paper on the 'Artificial Membrana Tympani.' A third edition of Mr. Lizars' work on 'Stricture of the Urethra' has been published.

In *Midwifery*, Dr. Meigs has reprinted, from the American 'Transactions,' his paper on 'Diseases of the Os Uteri;' Dr. Winn has edited, with notes, Dr. Conquest's 'Midwifery;' Dr. West has published the excellent 'Lectures delivered at the College of Physicians on Ulceration of the Os Uteri;' and a second part of their 'Clinical Treatise on Midwifery' has been issued by Drs. Chiari, Braun, and Spaeth.† Most of these treatises require special notice.

In *Physiology*, Mr. Gray's work on the 'Spleen' (Astley Cooper Prize) is by far the most valuable contribution to Physiology which the English school has produced for many years. The wonderful industry and ingenuity with which the inquiry has been carried out, have led to brilliant

* Des Métamorphoses de la Syphilis. Recherches sur le diagnostic des Maladies que la Syphilis peut simuler. Par Prosper Yvaren. Paris, 1854.

† Klinik der Geburtshilfe und Gynækologie. Von Drs. Chiari, Braun, und Spaeth. Zweite Lief. Erlangen, 1853.

results. But we must defer all comment till our next number. Another valuable work is one by Eckhard on the 'Nervous System.'* We have already given the pith of this work in our abstract of Eckhard's paper in the 'Archiv des Vereins für wissenschaft: Heilkunde;' but of course the author now discusses every point at much greater length, and follows out its bearings with greater care. It is an admirable treatise, and well merits translation. A short work on 'Diet,' by Donders,† expresses the latest doctrines of the Dutch school. It is of course somewhat anti-Liebigian in its tone, as might be expected from a countryman of Mülder and Moleschott.

In minute *Anatomy*, we have received the second volume of Mr. Quekett's admirable lectures on 'Histology.' Professor Gerlach, of Erlangen, has published a second edition of his treatise on the 'Anatomy of the Tissues.'‡ It is not so long as Kolliker's 'Handbuch,' and does not contain so many original observations. It is, however, written with extreme clearness, and its brevity will not render it less acceptable to students and busy men. The woodcuts are numerous, and illustrate the text sufficiently, though they are not distinguished by any great artistic merit.

In *Materia Medica*, besides Dr. Neligan's work, nothing has reached us except the first number of a serial, edited by M. Bouchardat.§ It is entirely occupied with the 'Memoir on Digitaline,' by MM. Homolle and Zueverme, which is, in great part, familiar to us as having been already published in other channels. The 'Archives' are to appear every four months.

In *Medical Jurisprudence*, we have received a treatise on the 'Signs of Death,'|| one on 'Insanity,'¶ and one on 'Criminal Lunatics,' by Dr. Hood.

Under the head of *Miscellaneous Subjects*, we may refer to a very interesting work, assigned, by rumour, to a most distinguished professional man, and which is entitled 'Psychological Inquiries.' As this work touches on many of the subjects which most deeply interest and agitate the minds of men, and as it is written with an amount of clearness and definition, to borrow a phrase from the language of microscopy, unusual in works of a like kind, our readers will perhaps thank us for entering into some discussion on its contents in an early number.

Two important Parliamentary papers have appeared—viz., the 'Report on the Health of the Navy' (Part II.), and an 'Account of the State of Disease in Ireland,' compiled from the documents collected at the census. These works will also be reviewed as soon as possible.

Dr. Seegen has written an interesting work on 'Mineral Springs.'**

* Grundzüge der Physiologie des Nervensystems. Von Dr. C. Eckhard. Giessen, 1854.

† Die Nahrungstoffe. Von F. C. Donders, Professor in Utrecht (aus dem Hollandischen übersetz, von Dr. P. B. Bergroth). Crefeld, 1853.

‡ Handbuch der Allgemeinen und Speziellen Geweblehre des Menschlichen Körpers, von Dr. Jos. Gerlach, Professor du Anatomie zu Erlangen. Mainz, 1854.

§ [illegible] de Physiologie, de Thérapeutique, et d'Hygiène, sous la direction de M. Bouchardat. No. [illegible]ier, 1854.

|| [illegible] Mort et de ses Caractères. Par le Dr. Josét. Paris, 1854.

¶ [illegible]tudes Med. Psychologiques sur l'Alienation Mentale. Par L. F. E. Renardin, M.D. Paris, [illegible]

** Die Naturhistorische Bedeutung der Mineral-Quellen. Eine Skizze. Von Dr. J. Seegen. Wien, 1854.

PART THIRD.

Original Communications.

Art. I.

Practical Observations on the Operation of Lithotomy. By John Crichton, Esq. (Communicated by William Sharpey, M.D., F.R.S.)

(*To the Editor of the British and Foreign Medico-Chirurgical Review.*)

University College, London, Nov. 1st, 1853.

Dear Sir,—The accompanying letter is from Mr. Crichton, the eminent surgeon, of Dundee, whose great success in operating for stone in the bladder is well known. It contains practical reflections on the operation of lithotomy, suggested by an experience of more than sixty years, and coming, therefore, with a weight of authority which, I should think, cannot fail to render them acceptable to your readers.

I am, dear sir, yours very truly,
W. Sharpey.

Dundee, Oct. 6th, 1853.

My dear Sir,—Two years ago, when, at your request, I forwarded to you the numerous urinary calculi you saw here which I had extracted from the bladder of patients suffering under calculus vesicæ, for the purpose of being analyzed, sawn across, the halves of each to be deposited in the museum of the London University College, and the other halves returned to me with the analysis of each, you will recollect I mentioned to you that I might very likely trouble you at some future period with such observations as might occur to me on the subject, in like manner as I had done some thirty years before upon forwarding about an equal number of calculi to Professor John Thomson, to be treated in a similar manner, and the halves deposited in the museum of the Royal College of Surgeons of Edinburgh. Other avocations, however, or rather, perhaps, my natural indolence of disposition, have hitherto stood in the way of performance. I am happy, however, to take this opportunity of again acknowledging how much I am indebted for the care and attention you bestowed upon the commission. The section of the calculi, by the application of steam power, through the kindness of Mr. Tomes, was most successful, and several of them are so variegated in their colours, and so beautifully polished, that if set in gold they might be used as personal ornaments.

The observations alluded to as being forwarded along with the calculi

to Professor Thomson, made their appearance in the 'Edinburgh Medical and Surgical Journal' for the year 1828, and a sequel to them in the same journal for the year 1837.

'Tis now upwards of threescore years since I first operated for stone in the bladder. A period so long in the prospect, so short to look back! How vivid and distinct every event which then occurred appears to my mind at this time. My marrying the object of my choice whilst still a stripling at school, considered by my friends an act of extreme folly, but by me then, and ever since, the perfection of happiness and the perfection of wisdom. My anxieties and cares at first settling in business. My being called to see a young boy, John Chalmers, suffering under calculus vesicæ, whose parents were kind enough to say that, having made themselves acquainted with the successful result of various serious surgical operations I had been engaged in, they had made up their minds to place their boy under my charge instead of taking him over to Edinburgh to be operated on, as at first it had been their intention, and which was the universal custom at that period. My hesitating at first to take upon me the responsibility of a case at that time considered by every one of so mighty serious a nature (for though married and father of a family, I was still not out of my teens), through fear that if the operation should not happen to be accompanied with perfect success, I might be blamed as rash and inconsiderate, and so injure the little reputation I had already acquired. My having recourse to Benjamin Bell's 'System of Surgery,' the standard work at that period. My being much puzzled to unravel the meaning of that passage in the section on lithotomy wherein he says, "When by a continuation of the incision the *erector penis and accelerator urinæ muscles* are also to be divided." My subsequently perusing with care every writer on the subject I could lay my hands on, and finding so many conflicting opinions and directions, and so much apprehension of danger to life from hæmorrhage, abdominal inflammation, and other casualties. My having at length recourse to the skeleton, and taking the bones of the pelvis in my pocket, examining, measuring, and comparing its boundaries as fixed by the rami of the pubes and ischium with its appearance in the boy himself laid upon a table in the position for operation, and with the sound in his bladder, definitively settling in my mind the direction of the incision, so as to avoid the bulb of the urethra and the rectum on the one side, and the pudic artery on the other; giving directions withal to the cutler to grind off to the extent of some lines from the breadth of the gorget to lessen any risk of its coming in contact with the pudic artery, and also to take away its round bulging shoulder, then in use, in order to give the cutting part more slope, so as to facilitate its entrance into the bladder. My proceeding to operate, on the 16th February, 1792, with my mind quite calm and confident, unappalled by the terror and screaming of the boy, and unconcerned as to what those around might be thinking. My delight in daily witnessing the rapid recovery of the boy, who, before the end of three weeks, was running about in great spirits and perfect health. My being then called upon to operate on James Valentine, ætatis 41, who had previously intended to go over to Edinburgh for that purpose, but had delayed his journey until the result of the case of the boy Chalmers should be ascer-

tained. The extraction from his bladder, on the 7th March, 1792, of a large rough calculus, weighing upwards of six ounces, and his subsequent recovery. All these circumstances, those days of happiness as well as of cares and anxieties, are so imprinted on my mind as to appear to me occurrences of yesterday, and never to be obliterated.

Without further detaining you, I now proceed to the promised observations, confining myself to what has actually occurred in my own practice, and under my eyes.

First, in regard to circumstances rendering an operation advisable or otherwise. The time has long passed since the operation was only considered admissible at certain seasons of the year, and upon patients under a certain age; but writers upon the subject, and of the highest repute, still object to submitting a patient to the operation unless the general state of his health is favourable to its success.

Sir Astley Cooper says in his lectures: "If the bladder is ulcerated, do not perform the operation on any account, for it will not be successful. But especially never submit a patient to the operation for stone if there be the slightest affection of the chest—the least difficulty of breathing—any sign of asthma, or any irregularity of circulation. No person who has any regard for the safety of his patient or his own reputation as a surgeon, will ever operate for a stone unless the chest be free from all complaint. You hear of one surgeon being exceedingly successful in the operation for stone, and of another less so. The cause of it is this: the one is careful to select his cases; he puts aside all those who have any other affection, and tells them to wait, and only submits those to the operation who are free from any other disease."

Many practitioners are too ready to be swayed by such dicta, and, through fear of an unsuccessful result, allow their patient to remain unrelieved. But what is to become of patients suffering under stone, who happen at the same time to have these objectionable affections? Are they to be allowed to linger out a miserable existence in torture, without hope of relief, crying for death to put an end to their sufferings? For my own part, I have never been able to reconcile such doctrines with humanity or professional duty, but from the first have always operated upon every case that presented, whether considered by others favourable or not. Neither have I found my professional reputation suffer thereby. On the contrary, I have, in various cases, experienced the satisfaction of seeing affections which were considered insuperable objections to an operation gradually give way after the pain and irritation occasioned by the stone was removed. A few instances, from amongst a number of others, will serve to illustrate this.

Case I.—Joseph Cairns, a child three years of age, brought to me in the month of April, 1800, by his mother, on her way from the Edinburgh Infirmary to her residence at Forfar, from whence she had conveyed him some days before to that place to be operated upon for stone in the bladder. After staying a few days in the infirmary, she was desired to take her child home again with her, as he was not considered a fit subject for operation. I took a room for them here, and removed the calculus in the usual manner. The urine was evacuated through the urethra from the first, the divided portion of the prostate adhering by the first intention without suppurating, and the child was taken home by his mother

to Forfar that day three weeks after the operation, free of complaint, and in good health and spirits.

Case II.—R. M., seven years of age, of a sickly, worn-out, emaciated appearance, residing at Crescent, a little way from town, who had some time before been sent into the infirmary here, suffering under symptoms of calculus vesicæ, but was dismissed by the surgeon then in attendance as not being in a state favourable for operation. His sufferings, however, becoming more and more urgent every day, I was requested by his parents to relieve him, if possible, at all hazards. A room was taken for him and his mother in town, that he might be more at hand, where I operated and removed the calculus. He made an excellent recovery, and returned home free of complaint and much improved in general health and strength.

Case III.—Mr. Peter Bruce, ætat. 45, residing at Errol, was brought, in the month of May, 1817, to a lodging prepared for him, wrapped up in blankets, and lying on a bed suspended by its four corners fastened to the posts of a cart. His countenance was ghastly in the extreme, and his flesh wasted away to a skeleton. He had been suffering for many years under symptoms of calculus vesicæ, and had been visited by several medical practitioners from this town as well as from Perth, who all agreed in opinion that no good could result from an operation, as the urinary organs, particularly the prostate gland, were all in a diseased state. Issues in the perinæum and verge of the anus, and medicines of various sorts, were prescribed, but without benefit, and his friends were ultimately informed that his case was utterly hopeless, and could only be palliated by opiates. For the last six months he had, in a great measure, been confined to bed, being unable to sit upright, constantly straining to void his urine in great agony, and, to all appearance, was fast sinking. Hearing, from various quarters, that I had been very successful in the treatment of similar disorders, he became exceedingly anxious to put himself under my charge, and had himself conveyed into town in the manner mentioned. Upon examination the following day, besides detecting a stone, I observed a great bulging of the bladder, compressing the rectum in such a manner as hardly to admit the passing up of the finger. After soothing and cheering him as much as possible by the hope of a speedy relief to his sufferings, I, a few days afterwards, extracted a large rough stone, weighing upwards of six ounces, which he bore with great composure. He passed the remainder of the day and following night quite easy and free of pain, the urine coming freely and plentifully by the wound without straining, and on the morning he took his food with a relish and lightness of heart he had long been a stranger to, and went on so well as to find himself able, on the eighth day after the operation, to be removed in a sedan chair to a friend's house in the suburbs, where he enjoyed superior accommodation, and a green to saunter about in, until he found himself sufficiently strong to return to Errol and attend to business. About a twelvemonth afterwards, happening to be in that neighbourhood, I called at his house, and found him looking so stout as hardly to recognise him. Both he and his wife received me with the kindest expressions of gratitude, withal setting before me a large dish of rich clotted cream, which they had heard I so much delighted in, and to which I certainly did all manner of justice.

Case IV.—William Powrie, æt. 45, was conveyed from the parish of Liff to the infirmary here, having been for many years affected with dyspnœa, palpitation of the heart, frequent attacks of asthma preventing his lying down in bed, cold extremities, &c., at same time suffering severely under symptoms of calculus vesicæ. He was detained in the infirmary two months, to ascertain if his sufferings could be any way removed by proper treatment, but the pain from the stone becoming still more urgent, and his other affections continuing unabated, it was decided to write to his friends to get him conveyed home as incurable. The poor man pleaded hard to be first relieved from the stone, whatever might be the consequence, but the surgeon then in attendance still refusing to operate under such

unfavourable circumstances, I offered to take the responsibility, which being assented to, I, on the 7th July, 1824, extracted a mushroom-shaped calculus with great ease by the lateral operation. He made a rapid recovery, and, what was particularly remarkable, after removal of the stone the objectionable affections gradually abated, and four weeks after the operation he walked home without assistance, a distance of five miles, in perfect health and spirits.

Case V.—James Richardson, æt. 45, was brought down from his residence in Perth by the steam-boat, bearing a letter from several benevolent persons there, recommending him to my care, and stating that they would be answerable for any expenses incurred. I went down with the messenger to the boat, and found a poor, emaciated, ghastly-looking object straining, with agonizing pain, to void his urine. I desired he should immediately be conveyed up to the infirmary; and learned from him that, eleven years ago, in consequence of a fall from a height, he had been affected with severe pain of the back and left side, accompanied with bloody urine, which confined him to the house for several weeks; since which period, he had several attacks of the same complaints, with the addition of passing quantities of sand and small stones to the amount of two hundred and seventy-five, many of them the size of large peas. He had also inguinal hernia of both sides, with cough, expectoration of muco-purulent matter, and dyspnœa, to such an extent as often to prevent his lying down in bed; and for the last twelve months had been suffering under symptoms of calculus vesicæ, which of late had been beyond endurance. Notwithstanding all these unfavourable accompaniments, I considered it my duty to operate, and on the 24th July, 1824, I easily extracted a rough flat stone, weighing 9 drachms, by the lateral operation, and two months afterwards he went home in great spirits, quite cured of all his calculous complaints, and nearly free of the affections of his chest. The following year, happening to be on a professional visit to Perth, he recognised me in the street, and told me, that soon after leaving the infirmary he felt himself able to resume his occupation, and had never been a day off work since.

Case VI.—James Wilson, æt. 74, brought to me from the parish of Cortachy, for the purpose of being operated upon for stone. Had been for the last five or six years subject to retention of urine, requiring the aid of the catheter. Twelve months ago, began to suffer under symptoms of calculus vesicæ, which for some time past had become extreme, straining every few minutes to void his urine, which came off intermixed with bloody muco-purulent matter, highly offensive to the smell. Is exceedingly emaciated, his countenance wobegone and sunken, his speech incoherent, and he is sometimes highly delirious. Pulse frequent and irregular. His case being somewhat uncommon, I delayed operating for some time; but as he was getting day by day more and more feeble, and becoming more and more unmanageable, his friends who came along with him became impatient; and, on the 25th August, 1830, I extracted a calculus weighing two ounces, with great ease.* The cure went on well, although the incoherence of speech, occasional delirium, and fœtid muco-purulent urine, continued for some time. By and by, however, matters began to improve, his urine coming off clear, and his incoherence of speech leaving him; and his appetite for food gradually improving, he was enabled to return home free of complaint, although somewhat feeble.

Case VII.—Andrew Davidson, æt. 56, had suffered long under symptoms of stone in the bladder, accompanied by dyspnœa to such a degree as to prevent him sleeping in a horizontal posture. On the 10th December, 1831, I removed two calculi, weighing together one ounce.† He made an excellent recovery, and was able for his work within a few weeks.

* Fusible calculus.

† Nucleus, oxalate of lime; succeeding layers, ditto; next, neutral phosphate of lime; then phosphate of lime, with traces of triple phosphate.

Case VIII.—Mr. Robert Ferguson, æt. 54. I was requested to intimate, by return of post, the first day I could make it possible to proceed to Muirhous-Law, in Roxburghshire, to see Mr. Ferguson, and advise with his medical attendants and friends how far it might be practicable and justifiable to submit him to an operation for the removal of stone in the bladder, in his worn-out and apparently dying state. I met with them all there towards noon, on the 6th July, 1843, and found Mr. Ferguson in a most pitiable condition indeed; his countenance was sunken and his looks ghastly, his body emaciated in a remarkable degree, his mind in some degree unconscious, and at times delirious, straining every now and then to void his urine in great agony, his stomach generally rejecting everything he was made to take except tincture of opium, which he swallowed in great quantity. I said, if anything was to be done it must be so immediately, as he was evidently fast sinking. Preparation was accordingly made, and, that same evening, I extracted, with some management, requisite on account of the thickened and enlarged state of the prostate gland, a rough stone, weighing two ounces. He appeared quite unconscious during the operation, and for several hours afterwards spoke incoherently. About ten o'clock he fell quiet, and slept till next morning; he awoke much refreshed with his long sleep, and took some food, which remained on his stomach. Throughout the day he was quite calm, taking frequently a little biscuit soaked in wine, with a relish and in good spirits, the urine flowing plentifully by the wound. On the following morning, finding he had passed a very calm and pleasant night, and everything going on well, I left him under the care of his medical attendants, and was happy afterwards to learn that he passed his urine altogether by the urethra on the tenth day, and upon the twenty-first was walking about in the open air, rapidly recovering his strength and flesh. Every Christmas since, he has not failed to keep me in remembrance of him by forwarding a small box filled with game and fat ducks of his own rearing, the latter of which he had learned I was peculiarly fond of.

Case IX.—John Simpson, æt. 38, a poor, emaciated, ghastly-looking object. I was informed that he had been suffering more or less for seven years under the usual symptoms of calculus vesicæ; that latterly his sufferings had become so urgent as to disable him for work; and for some time past he had been unable to leave his bed—that he had been seen and examined by several medical practitioners, who could not detect any appearance of stone, and had all given their opinion that the urinary organs were all in a diseased state, for which there was no cure, and which could only be palliated by opiates and other sedatives. I sounded him, and, after some little search, distinctly struck a stone towards the right side of the bladder. Upon afterwards repeatedly sounding him, along with other professional gentlemen, the calculus was invariably found at the same place, fixed and immovable, which led to the conclusion that it must certainly be partially encysted. Having previously met with cases of a similar nature, and experienced much difficulty and delay in getting the encysted portion disentangled, it became a matter for consideration how far, in his worn-out and almost moribund state, an operation should be had recourse to, lest he should die amongst our hands. Upon the matter being explained to him and his wife, he faintly replied, by all means relieve him, if possible, of his unbearable sufferings, whatever might be the consequence. Accordingly, on the 16th May, 1844, an opening was made into the bladder in the usual manner, and the presenting part of the stone quickly grasped and extracted by the forceps, broken off at its slender neck; but it was not without much difficulty and perseverance that the encysted portion was disentangled from its pouch, which was at length, however, safely accomplished, chiefly by cautiously introducing a female grooved staff through the narrow aperture, and gently and gradually making it find its way by the side of the encysted portion till it reached its base, supporting the poor man from sinking during the while by frequent sips of brandy and water. He made an excellent recovery; and by means of nourishing diet, with a moderate supply of wine and ale, he soon regained his strength and flesh.*

* Fusible calculus, with uric acid.

The above few cases, taken indiscriminately from amongst many similar ones, may be sufficient to show that neither the safety of the patient nor the reputation of the surgeon has been compromised by listening to the dictates of humanity and professional duty. In justice, however, to Sir Astley Cooper and others who maintain that only select cases should be submitted to an operation, I shall now instance a few cases wherein the operation, although successful in itself, failed in also removing the accompanying objectionable affections.

CASE I.—James Donaldson, æt. 5. When first called to see this child, towards the commencement of the year 1812, I found him of a delicate scrofulous habit, with tumid belly, painful upon pressure, and other symptoms denoting affection of the mesenteric glands; and, at the same time, labouring under symptoms of calculus vesicæ. I advised the removal of the stone in the first place, which, by relieving the pain and irritation therefrom, would afford a better chance of restoration to general health. The parents, particularly the mother, would not give ear upon any account to the proposal, and I lost sight of the case till about six or eight months afterwards, when I was again called upon to visit him. His sufferings from the stone had become latterly much more urgent; the mother was worn out by constant watching and witnessing the incessant agony of her child, and both parents were now as importunate to have him operated upon as they had formerly been adverse; but the great emaciation, the tumid and tympanitic abdomen, rapid pulse, &c., showed evidently the child was fast sinking under tabes mesenterica. Nevertheless, I could not resist their importunities, and accordingly, on the 9th October, 1812, the calculus was removed safely and expeditiously, and everything went on well as regarded the operation, but the tabes still progressed, and he soon afterwards sunk under it.

CASE II.—Charles Duncan, æt. 66, was conveyed here from the parish of Cortachy, to be operated upon for stone, under symptoms of which he had been labouring for five years. He was much emaciated; his appearance, wobegone, sunken, and ghastly. No appetite for food, and rejecting anything he did take; and so feeble, as to be unable to sit upright. Of late his sufferings had become unbearable; and hearing that many from his part of the country, under similar complaints, had returned from Dundee quite cured and well, he became exceedingly urgent to get there, and set off in a cart stuffed in the softest manner, accompanied by his wife and one of his friends; but they were frequently under the necessity of halting by the way, fearing he would not be able to accomplish the journey. For eight or ten days he was kept quiet, and every means used to palliate his sufferings and improve his digestive powers, but without effect; he was getting more and more feeble and worn out every day. To reconvey him home alive was out of the question; and without removing the immediate cause, nothing could be effected. Accordingly, on the 13th July, 1812, a large rough stone was easily enough extracted; but his powers of life were too far gone to be restored, and he sunk without pain within three weeks afterwards.

CASE III.—Mr. David Small, æt. 66, had been for many years labouring under symptoms of calculus vesicæ; but instead of submitting to an operation for relief, he tried to palliate his sufferings by means of opium, which he made use of to a great extent. He was subject to frequent fainting-fits, out of some of which it was not expected he could recover. Latterly losing all appetite for food, with much thirst, his pulse frequent and irregular, and his strength every day falling off more and more, whilst his sufferings were becoming more intense, he at length made up his mind to submit to an operation, and in March, 1816, two calculi were with much ease extracted. About two hours afterwards, he was seized with an apoplectic stroke, accompanied with stertorous breathing for eighteen hours,

after which he revived a little, and became somewhat conscious; but soon after relapsed, and died comatose.

CASE IV.—Mr. Finlay, æt. 69, was conveyed here from beyond Perth, for the purpose of being operated upon for stone, under symptoms of which he had been suffering for five years. His case altogether was most discouraging. His pulse was frequent, irregular, and intermitting. He was subject to frequent fainting-fits, threatening his life; constant dyspnœa towards evening, rendering it necessary for him to be supported in an arm-chair during the night, instead of lying down in bed. He had much thirst, and no appetite for food; constantly straining, with great pain, to void his urine; swallowing laudanum in great quantities, out of a quart bottle he brought along with him, to afford a little temporary relief, &c. It was explained to both himself and friends, that there was great risk of his going off in one of his fainting-fits during the operation, if he still persisted in undergoing it; but he replied, his mind was perfectly determined to have the stone, which was occasioning him so much torture, removed at all hazards. Accordingly, on the 1st October, 1816, while he was supported on the table in nearly an erect position, a large rough stone was extracted with great ease, although the prostate gland was considerably enlarged. He bore the operation with the greatest composure; and when I saw him in the evening, I found him quite happy in being relieved from his agonizing strainings, the urine coming off by the wound freely and abundantly, and he had taken some little refreshment; but the dyspnœa returning, as usual, about that time, he found it necessary to get out of his lying posture, and support himself in his arm-chair beside the fire. In the morning, I found he had enjoyed a fine easy night, accompanied by much refreshing sleep; had taken his breakfast with a relish, and had walked across the room, supported on the nurse's arm, before lying down in bed. He continued to improve rapidly, taking food with a relish, and in great spirits, lying in bed during the day, and supported in his arm-chair during the night; till one day, walking across the room, leaning on the nurse's arm, he fell down in one of his fainting-fits, and instantly expired.

CASE V.—James Barney, æt. 60, had been labouring under symptoms of calculus vesicæ for ten years, and about that period had made up his mind to submit to an operation; but after being laid upon the table, his resolution failed him, and he took to the use of alcohol and opium for relief. During the last four months, his sufferings had become intense; his urine was loaded with fœtid, bloody, purulent matter, and he was subjected to frequent attacks of epilepsy. Becoming desperate from the intensity of his sufferings, and death staring him in the face, he peremptorily insisted upon the removal of the stone, whatever might be the consequence. I accordingly operated in the month of September, 1819, and the stone appearing to be of immense size, I made use of a very strong pair of forceps, with which I succeeded in breaking it into several pieces; the whole, when put together, weighing nine ounces, the largest portion weighing six ounces. He passed that day and night pretty composed; but the following evening, after some sort of strangling and screaming, he became comatose, and died under symptoms of oppressed brain.

CASE VI.—Henry Whyte, æt. 62, was conveyed from Fife to the Infirmary here, for the purpose of being operated upon for the stone. Nine years ago had a severe attack of pneumonia. The continuance of cough and dyspnœa had disabled him for work during the following three years; and for the last twelve months, he had been under the necessity of confining himself to the house, any exposure to the air aggravating his dyspnœa and cough. He had also a large [illegible] tumour hanging half-way down his right thigh, and behind that the scrotum was tremendously enlarged by hydrocele, containing upwards of two pounds of serum, as appeared when afterwards drawn off. The surgeon then in attendance, understanding that the poor man's friends had expressed their anxiety that I

should operate, he consigned him over to my charge; and although the case was far from favourable, yet as his sufferings from the stone had latterly been agonizing beyond endurance, I proceeded to operate on the 8th October, 1838. Upon introducing the forceps and grasping the stone, I at once perceived, by the immense separation of the handles, that the extraction in that position would be quite impossible. I accordingly withdrew them, and by means of my finger and a scoop, succeeded in altering its position, so that, in the next grasp, the handles were much less separated, and with some little management, the removal was safely accomplished. The stone was very large, being 3⅝ inches in its largest diameter, and 2½ in its shortest; 8⅝ inches in its largest circumference, and 6 inches in its shortest.* In regard to the operation itself, everything went on perfectly well; the urine flowed freely and plentifully by the wound; he suffered neither pain nor uneasiness in the pelvis or abdomen, affording every appearance of a rapid recovery; but the cough and dyspnœa still continued, accompanied with pain, tightness and oppression of the chest, all which hourly increasing, he breathed his last on the fourth day afterwards.

Without making any comment on the foregoing cases, I now proceed to remark,

Secondly, upon the *Operation*. At the period of my commencing practice, in the year 1791, the methods previously practised in operating for the stone, as fully detailed, and modestly commented on, by M. Deschamps, in his excellent 'Traité Historique et Dogmatique de la Taille,' had all, in a great measure, given way to Cheselden's, under the designation of the lateral method, the cutting gorget of Sir Cæsar Hawkins being used in dividing the prostate.

This instrument, modified, as I before mentioned, by narrowing the breadth, and giving greater slope to the cutting part, I made use of from the first, and continued to do so for a period of years, and with perfect success, notwithstanding the rampant denouncement of it by Mr. John Bell, in his elaborate quarto volume on lithotomy, designating it "A murderous weapon," and adding, "the gorget slips! and all the surgeons of Europe confess it! It slips in the hands of the most skilful surgeons, and no one can be responsible for the consequences of a thrust so desperate, and requiring so much force. It slips so frequently, and is avowedly so little under the control of the operator, that no man ventures to blame his brother for a misfortune which may happen under his own hand. I have myself seen it driven, God knows where, deep out of sight, up to the hilt, without one drop of urine issuing, without the operator ever reaching the stone." And so on in the same style throughout several pages of his fourth volume.

When I first perused all this, is it possible, thought I, that I should have been making use of this murderous weapon for so many years without being in the least aware of its murderous propensities? If the beak of the instrument slips out of the deep groove of the staff, in the hands of the most skilful surgeons of Europe, how is it that I have never, for one, found it to do so? But I soon perceived it was only his usual exaggerated manner of expressing himself, and that he might, in like manner, have proscribed the use of the knife, as he, without doubt, must have witnessed a dozen of cuttings made with it to get into the groove

* Nucleus and body of the calculus, uric acid and urate of ammonia; external crust, oxalate of lime and uric acid.

of the staff, instead of one; or the use of the forceps, as he must also have witnessed an hour and more occupied in fruitless attempts with them to grasp and remove the stone.

In a correspondence with Mr. Martineau, of Norwich, some time before his death—who, it would appear, along with many others, had been frightened out of his propriety at the denunciations of Mr. John Bell—I was not a little amused at the *naïveté* of his statement, that he had been very unsuccessful in his operations whilst he used the cutting gorget, but was successful when he gave it up.

Certainly, there were objectionable points in the original form of the cutting gorget, which I caused to be remedied before making use of it; and, with these alterations, I have never found it requiring the smallest force for its introduction into the bladder, never slipping out of the groove of the staff, or in the least endangering the pudic artery.

Latterly, however, in boys, and those who do not appear to have a deep pelvis, or enlargement of the prostate, I have dispensed with the use of it, carrying the same knife, after penetrating the urethra at the apex of the prostate, straight onwards, directed and supported by the index finger of the left hand, thus rendering the operation more simple in the saving of time by lessening the number of instruments, and enabling me often to have the stone in my hand within the minute.

But, in those having a deep pelvis, with much hypertrophy of the prostate gland, rendering the bladder inaccessible to the finger, I think it most prudent to revert to the use of the gorget, or probe-pointed bistoury.

Notwithstanding the numerous and contending directions of writers on the subject, as to the finding and grasping the stone, as if it were a most serious and difficult matter to accomplish, yet, excepting in partially encysted calculi, or in very small ones perhaps enveloped in some of the folds of the mucous membrane, I have always found the stone where, from the previous contraction of the bladder, it most naturally should be found, close by or behind the opening, and easily laid hold of and extracted; but, in the partially encysted, I have had my patience and perseverance sorely tried in getting the encysted portion safely dislodged without injury to the bladder; and, in very large ones, every one knows the difficulty and management required in the extraction. It has only been in two cases, however, that I have been under the necessity of breaking the stone previously to extraction—viz., James Barney, whose case has been already stated; and the other, a patient of Dr. Mudie, in Arbroath, during the summer of 1793, who had requested my assistance, wherein the widely expanded handles of the forceps, in grasping the stone, showed to us both the impossibility of extracting without first breaking it. Accordingly, applying my whole strength to the handles, the stone fortunately gave way, splitting into two nearly equal halves, which were immediately extracted separately, and, when put together, weighed fully ten ounces. The patient made a perfect recovery, without the smallest interruption to the cure.

But, however easy I have always found the grasping and extracting calculi, of moderate size, from the bladder, yet I have, upon various occasions, witnessed surgeons, of long standing and high repute in their

profession, unaccountably lose their self-possession in this simple stage of the operation, of which I shall give an instance or two.

In the winter of 1790–91, a middle-aged man, in the Edinburgh Infirmary, had to be operated on by Mr. ——, one of the most eminent surgeons of Edinburgh during the last century. Being a dresser in the surgical ward, I had the privilege of being beside the patient during the operation, and, as far as could be observed, the operator went through the first stages with composure, but after introducing the forceps, and turning them in every direction—opening them and shutting them, shutting and opening, for a great length of time—without grasping anything, he was observed becoming very much agitated, and no way bettered by the half-suppressed hissing and other marks of impatience amongst the students in the gallery, when old Mr. Wood, one of the most princely and kindly-hearted gentlemen of the profession, as gently and unobservedly as possible, took hold of the forceps, grasped the stone, and placed them again in Mr. ——'s hands, who immediately lost hold of it, and returned to his searching—opening and shutting, until Mr. Wood, seeing that the operator's self-possession and presence of mind had quite forsaken him, a second time took hold of the forceps, and withdrew them, with the stone in their grasp, to the external part of the wound. The patient died within an hour or two after being put to bed.

Another case, of a later date, took place in our infirmary here, in which the surgeon then in attendance, after a long time groping with the forceps in the bladder—opening and shutting them again and again without being able to grasp anything—the sweat rolling down his face in big drops—in his great agitation at length requested me to ascertain, if possible, the cause. Not feeling any calculus, either by my finger or a female sound, and judging that, if a small one, it might possibly be enveloped and concealed in a fold of the mucous membrane, I filled a large syringe with tepid water, and injected the fluid through the wound into the bladder; by the distension of which the small stone was disentangled, and forced, by the returning gush, towards the neck, where it was immediately grasped and extracted. The patient made a good recovery.

I have also witnessed much unpleasant cutting, in the endeavour of the operator to get the knife inserted into the groove of the staff, perhaps from his thoughts being occupied in what those present might be thinking of him, or from the want of clearness of conception in his mind in regard to the situation of the parts to be divided; and, oftener than once, I have even been requested to assist in the accomplishing it.

Third. *Hæmorrhage.*—Notwithstanding the many instances recorded of death arising from hæmorrhage, and the great apprehensions formerly, and even still entertained, by writers on the subject, yet in operating upon about 200 cases, during a period of upwards of threescore years, I have never once experienced such a distressing occurrence, and am rather inclined to think that, except in some anomalous distribution of the vessels of the perinæum, such an event cannot possibly occur when the incisions are properly conducted.

Fourth. *Abdominal Inflammation and Urinary Infiltration.*—These have likewise been held forth as a frequent cause of death, arising from

the operation; but I have never experienced any such casualties in my own practice, excepting in the case of one patient in our infirmary here, who died from peritonitis after I had operated on him, but who had been a sufferer from some supposed organic affection in the pelvis for many years previously to his having become affected with the symptoms of calculus.

I have heard, however, of such occurrences, especially urinary infiltrations; and in one case, where I was a spectator of the operation, and had occasion to see the patient some days afterwards, I observed the wound full of a gangrenous cellular substance, and the testicles quite bare, from the sloughing off of the scrotum in consequence of urinary infiltration.

Fifth. *Healing by the first intention.*—Amidst the numberless writings upon the subject, it is not a little remarkable that adhesion, union, or healing by the first intention, of the divided portion of the urethra and prostate gland, has hardly ever been alluded to; yet, in my own practice alone, twenty-three instances of its taking place have occurred, most of them particularly stated in the 'Edinburgh Medical and Surgical Journal' for the year 1837, whereby annoying and vexatious scaldings and festerings of the surrounding integuments, from the incessant dribbling of urine by the wound, have been avoided, and the cure itself so remarkably accelerated, that in patients from under two years of age to upwards of seventy, I have seen the former crawling about upon hands and knees, and the latter walking about the room, within a few days after the operation.

It is only, however, in patients of good habit of body, the stone of moderate size and easily extracted—where the urethra and prostate have been recognised at the first incision, and where repeated ineffectual cuttings have *not* been found necessary to get the beak of the gorget or other instruments fitted into the groove of the staff—that such a happy occurrence may reasonably be looked for.

Upon various occasions I have been asked whether I employed any particular method or instrument in operating, which might account for the continued successful results; and if so, why not make it public? I could never say I had any particular method. But perhaps a short statement of the last case that occurred, and which is fresh in my recollection, may best serve as an answer. This took place on the 7th of May, this present year, in a delicate boy, eight years of age, who had been affected, more or less, with symptoms of calculus from his earliest infancy, but latterly his sufferings had become so extreme as to induce his parents to submit him to an operation for relief. After introducing the staff—which, I must say, was held most steadily in the proper situation by your friend Dr. Arrott—I commenced the incision deep in the perinæum, by the side of the raphé, eight or ten lines in front of the anus, gradually lessening its depth as it passed between the anus and tuber ischii onwards to its termination; then introducing the forefinger of my left hand into the deep superior part of the wound, and pressing its point in front of the apex of the prostate, I pierced the urethra with the knife, and carried it onwards in the groove of the staff, directed and supported by the finger, so as to divide the prostate obliquely outwards, and downwards nearly in the direction of the external wound. Next, feeling the stone with my finger, I withdrew the knife, and introducing the forceps, I

withdrew the staff,—all which was the work of a few seconds. But a difficulty now occurred, for after grasping the stone, and attempting its removal, it slipped from between the blades. The same occurrence taking place after two or three seizures, I judged there must be something uncommon, and, withdrawing the forceps, I introduced my finger in order to ascertain the cause, and found the stone, as before, close under the opening, but extending further backwards than my finger could well reach. Instead, therefore, of reintroducing the forceps, I made use of the scoop, passing it onwards till it reached the further extremity of the stone; then insinuating my finger gently underneath the fore-end, I raised it up to the opening, and, with the bent end of the scoop behind, drew it easily forward, and in a moment the stone was in my hand.

The boy made a rapid recovery, and soon acquired flesh and strength. Upon examination the stone was of an unusual shape, measuring four inches in its longest circumference, and one and three-fourths in its shortest. The two or three minutes occupied in manipulation with the finger and scoop was abundantly recompensed by the easy and safe removal of the stone, which, on account of its position and great length, could hardly have been accomplished by the forceps without breaking it or injuring the bladder.

Although the operation as here described appears the most simple, easy, and least painful of any surgical operation, requiring only two cuts with a sharp smooth blade to make an entrance into the bladder—and if the position and length of the stone had not stood in the way, would have been easily and safely accomplished within half a minute, for I have often before accomplished the same within the minute—yet, when we hear of, or witness, a surgeon, even in quite a favourable case, losing his self-possession and presence of mind, from the occurrence of some real or imaginary difficulty, his thoughts perhaps more occupied in what those around him are thinking, than in calmly and coolly exercising his judgment in what manner he may best overcome the difficulty, at length, in desperation, using whatever instrument he may have in his hand in such a manner as to occasion serious and lasting injury to the parts, so as even to endanger life, we are ready to conclude that Sir Astley Cooper's 'Selection of Cases,' or Sir Charles Bell and his brother John's injunctions that no one should operate on the living without previous and frequent dissections of the dead, are not the sole requisites to render the surgeon a successful operator. But in an operation such as this, unaided by the eye, and dependant upon a deep and just conception of the whole of the concomitant circumstances, more previous intentness of thought and abstraction of mind, leading to calmness and self-possession during the operation, are necessary for a continued successful result, than the generality of surgeons engaged in the routine of a lucrative practice are able or willing to bestow.

In regard to myself—as before mentioned, previously to making up my mind to undertake the responsibility of operating upon the boy Chalmers—after bringing to my recollection the nature and situation of the parts concerned as observed in the dissecting-room, as well as the operations I had witnessed in the Edinburgh Infirmary, and perusing every work upon the subject I could lay my hands on, till at length by minutely examining the

perinæum of the boy with the sound in his bladder in the position for operation, I got my mind disentangled from the conflicting opinions and directions of writers on the subject and formed my own opinion. I fixed and settled upon the only way of conducting the incisions so as to obtain a sure and safe entrance into the bladder, and proceeded to operate with my mind cool and collected, intent only on the object in view. And finding the result consonant with my previously formed conception, the boy making a rapid cure, without the smallest symptom of an unfavourable nature, I gained additional confidence; and, during threescore years' practice, operating upon about two hundred patients from two to eighty-five years of age, it is now a cheering reflection, and for which I cannot be sufficiently grateful, that with the exception of the six cases already made particular mention of, who died at shorter or longer periods afterwards, in consequence of concomitant organic disorders of the head, thorax, or pelvis: and eight other cases, accompanied with similar organic affections, not considered necessary to particularise here, every one else of the whole number, many of them too of a very unfavourable character, made excellent cures.

In regard to lithotrity, at first so much vaunted of as altogether superseding the use of the knife, I have little to say, beyond some remarks which made their appearance in the 'Edinburgh Medical and Surgical Journal' for 1837.

I shall relate now two or three cases which rather tend to corroborate the opinion I had then formed. Captain ——, of the royal navy, who had come in August, 1824, from Alnwick in Northumberland, to put himself under my care, had been for twelve years affected with dyspnœa and frequent attacks of nephralgia, followed by the passing of small calculous concretions, and for eight months previously to his coming here had also been suffering under calculus vesicæ, which latterly had become so urgent as led him to make up his mind to go up to London to be operated upon by Sir Astley Cooper, but he departed from his first intention, and came down here. Upon sounding him after his arrival, a stone was distinctly felt, but rather unfortunately one of the medical gentlemen present said in his hearing, that as the calculus would likely be small in consequence of the shortness of the period since the symptoms had made their appearance, it might readily be extracted without cutting, by means of Weiss's newly-invented forceps. We met next day for that purpose, and after introducing the forceps, and commencing gradually to open the blades by means of the screw attached to the handle, he groaned in agony, and got sick and about to faint; the screw was instantly unturned, and after some interval the attempt to expand the blades was again cautiously made, when the same expressions of extreme agony, accompanied by rigors, again took place, upon which it was agreed that no further attempts should be persevered in. The rigors continued for some hours, followed by sharp fever, and it was not till eight days afterwards that he was in a state to be operated on, when a rough stone, weighing six drachms, was easily extracted by the lateral method. He bore the operation with great composure, and when he got into bed said, that if the same should ever again be found necessary, he would submit without the smallest hesitation, but preserve him from Weiss's instrument of

torture. He made an excellent recovery, and soon afterwards embarked in a government yacht for Alnwick. Some years afterwards, upon a visit here, he told me, that with the exception of occasional slight returns of dyspnœa, he had enjoyed perfect health since the operation, having never experienced the slightest return of nephralgia, or the passing of calculous concretions.

Some time afterwards, I learned that a respectable gentleman in this place lost his life within a day or two, in consequence of the practitioner persevering in his attempts to crush and extract the stone in his bladder without cutting. And subsequently to this, I was requested to attend at the extraction of a stone from the bladder of a female by distension of the urethra, and witnessed a scene of suffering of an hour's continuance such as I can never think of without horror, considering that the whole might have been easily and safely accomplished within a few seconds by merely making a slit in the urethra.

Thus, my dear sir, I have now fulfilled my promise of giving you in writing such observations and recollections on the subject of lithotomy as have occurred to me during a practice of upwards of sixty years, and in as condensed a form as I possibly could: and if you should consider the matter therein contained to be in any measure worthy of the attention of the profession, particularly of the younger portion, you are at liberty to use your own discretion as to the publication of this letter.

Permit me to conclude in the words of the celebrated Cheselden, at the end of his short historical account of cutting for the stone:—"If I have any reputation in this way, I have earned it dearly, for no one ever endured more anxiety and sickness before an operation: yet from the time I began to operate, all uneasiness ceased; and if I have had better success than some others, I do not impute it to more knowledge, but to the happiness of a mind that was never ruffled or disconcerted, and a hand that never trembled during any operation."

I am, with much respect, my dear sir, yours very truly,

JOHN CRICHTON.

Dr. Sharpey, 35, Gloucester Crescent, London.

Art. II.

Scarlatinal Dropsy. By John W. Tripe, M.D.

(Continued from No. 25, p. 248.)

In the previous article, we have considered our subject in its general details, including the various circumstances which modify the invasion, progress, and fatality of scarlatinal dropsy; the alterations effected in the blood and urine, and also the set of symptoms classed together under the name of uræmia. In the present article, we propose to investigate, as fully as our limited space will admit, the special peculiarities, diagnosis, prognosis, and treatment of scarlatinal anasarca. As before mentioned, I have included under the head of scarlatinal dropsy all cases of effusion into the cellular tissue, or into any of the cavities, and also all cases of uræmia which were registered in connexion with scarlet fever. It may be considered, in the present state of our knowledge, that I am taking a retrograde course in adopting the title of dropsy, and thus elevating a symptom to the rank of a disease; but as any other name would have expressed some theory, I have preferred using a well-known and time-honoured name. It may also be objected that I should not have included, under the name of dropsy, cases of uræmia in which there was no effusion; but as the uræmic symptoms supervened after an attack of scarlatina, I consider myself justified in adopting this course: and an examination of the returns of the registrar-general shows that these cases are of far greater frequency than is usually supposed, for death from convulsions, at a late period of the fever, is by no means uncommon. There were many cases registered in the year 1848, in which no secondary disease was mentioned save convulsions, although the duration of the disease was stated at a fortnight or three weeks. I have seen several cases of the kind myself, and must acknowledge, if the patient be then first seen, that the diagnosis is attended with much difficulty; but in all my cases, an examination of the urine asserted, in language too plain to be misunderstood, that the patients were suffering from renal disease.

The fluid effused into the cellular tissue and cavities has been shown to vary considerably in its chemical composition. This variety depends not only on the seat of the effusion, but also, apparently, on difference of constitution, or other individual peculiarity, as the fluid differs in the corresponding cavities of different individuals. It also frequently shifts its place of deposit, leaving one part to be effused into another, but most frequently invades the eyelids first, and departing last from the feet or ankles; sometimes, however, it lingers longest in one of the cavities.

The day of invasion varies considerably, occurring sometimes during the eruptive period of the primary disease, and in others not until after the lapse of six weeks or more from its outbreak. Most authors consider either the twentieth, twenty-first, or twenty-second day from the commencement of the disease to be that on which the effusion usually first manifests itself, but an examination of my own cases, and of the returns of the registrar-general, shows this opinion to be incorrect; for both point to the fourteenth day from the commencement of the febrile stage as that on which the dropsy most frequently occurs. To elucidate this point, I

have analyzed 41 cases which occurred in my own practice, and the returns for the year 1848:

TABLE XVI.—*Scarlatinal Anasarca. Day of Invasion, for the Year* 1848.

Reports of Registrar-General. 1st Class.			Case Book. 2nd Class.	
Day of Scarlatina.	Total No. of cases.	Per cent.	Total No. of cases.	Per cent.
1st	2	·62		
2nd	2	·62	2	4·9
3rd	2	·62	1	2·4
4th	1	·31		
5th	2	·62		
6th	4	1·24	1	2·4
7th	18	5·56	3	7·4
	—= 31	—= 9·59	—= 7	—= 17·1
8th	2	·62	1	2·4
9th	3	·93	4	9·8
10th	9	2·78		
11th	10	3·10	1	2·4
12th	10	3·10	6	14·7
13th	19	5·87	1	2·4
14th	70	21·67	6	14·3
	—= 123	—= 38·08	—= 19	—= 46·4
15th	16	4·95		
16th	4	1·24	3	7·4
17th	12	3·71	1	2·4
18th	17	5·26	1	2·4
19th	5	1·55	1	2·4
20th	18	5·57	1	2·4
21st	40	12·38	5	12·3
	—= 112	—= 34·66	—= 12	—= 29·3
22nd	6	1·86	1	2·4
23rd	3	·93		
24th	3	·93		
25th	6	1·86		
26th	4	1·24	1	2·4
27th	2	·62		
28th	13	4·03		
	—= 37	—= 11·47	—= 2	—= 4·8
29th	0	...		
30th	4	1·24		
31st	1	·31		
32nd	2	·62		
33rd	1	·31		
34th	1	·31		
35th	5	1·55		
	—= 14	—= 4·34		
5 to 6 weeks	3	·93		
6 to 7 weeks	2	·62	1	2·4
7 to 8 weeks	0			
8 to 9 weeks	1 = 6	·31		
Totals	323	100·00	41	100·0

An examination of this table shows that the fourteenth was the day on which the dropsy most frequently came on, and that, therefore, the

opinion entertained by Dr. Copland and others, that the twenty-second, twenty-third, and twenty-fourth days are those on which, after the twenty-first, it most commonly happened, is erroneous. On contrasting, however, the results obtained by an examination of 41 attacks with 323 deaths registered in the returns of the registrar-general, we find rather opposite results: 17·1 per cent. of the former came on within seven days after the commencement of the scarlet fever, and only 9·59 per cent. of the latter series of cases. (This difference may be accidental, as these 41 cases include all those which I ever remember to have had at so early a period of the disease, and exclude very many which supervened at a later period, as I have not kept a record of all my cases; these having chiefly occurred during the year 1848.) A larger number of cases also —46·4 per cent.—occurred in the second class during the second week, to 38·08 per cent. in the first class. There is also a variation of an opposite character during the third week, when 29·3 per cent. occurred in the second class to 34·16 per cent. in the first; and only 4·8 per cent. in the second class during the fourth week to 11·47 per cent. in the first. But these discrepancies may arise from the small number included in the second class, in which each case represents 2·4 per cent. of the whole, whilst in the first it represents only ·31 per cent. It is therefore evident, that an accidental occurrence of a few cases at a particular period in the second class would vitiate all the conclusions. Still the results drawn from these two opposite and independent sources lead to certain uniform conclusions, which will be presently stated.

On proceeding to a more accurate examination of the first class, we find it indicates that less than 1·0 per cent. of the dropsy occurs on any given day during the first five days of the parent disease, 1·24 per cent. on the sixth day, and 5·56 per cent. on the seventh; making a total of 9·59 per cent. during the first week. We find the proportion again to fall during the eighth and ninth days, to rise *gradually* to the thirteenth, when it reached 5·87 per cent., and then, *suddenly*, on the fourteenth, to 21·67 per cent. (which was by far the highest), making a total, during the second week, of 38·08 per cent.; or of 47·67 per cent. during the first fortnight from the commencement of the fever. It also indicates that 34·66 per cent. of attacks happened during the third week, and 12·38 per cent. on the twenty-first day; that 11·47 per cent. supervened during the fourth week, 4·03 occurring on the twenty-eighth day; that 4·34 per cent. came on during the fifth week, ·93 per cent. during the sixth week, ·62 per cent. during the seventh week, and ·31 per cent. during the ninth week. The cases in the second class indicate the seventh, ninth, twelfth, fourteenth, and twenty-first days, as those on which the disease most frequently shows itself; and those of the first class, the seventh, thirteenth, fourteenth, eighteenth, twentieth, and twenty-first days. Of the 323 *deaths*, 59·41 per cent. took place on the seventh, twelfth, thirteenth, fourteenth, eighteenth, twentieth, and twenty-first days; and 56·3 per cent. of the *attacks* also happened on the same days. *We may therefore state that the fourteenth day from the commencement of the fever is that on which the invasion of scarlatinal dropsy most frequently happens; and that the other days on which the invasion most frequently occurs, are the twenty-first, twelfth, and seventh, the order of frequency being as they are here placed; and then the thirteenth, eighteenth,*

and twentieth respectively; these latter presenting but slight variations as to frequency.

The *duration of the dropsy*, although a point of some importance as regards prognosis, is one which has not been hitherto statistically considered. The disease is usually looked on as one of rather a chronic character than otherwise, unless it prove fatal in the first stage. The following table places this point on a certain basis.

TABLE XVII.—*Scarlatinal Anasarca. Duration before Death.*

Duration.	Total No. of deaths.		Per cent.	
1 days	5		1·1	
2 ,,	9		2·0	
3 ,,	14		3·1	
4 ,,	16	140	3·5	31·0
5 ,,	18		4·0	
6 ,,	24		5·3	
7 ,,	54		12·0	
8 ,,	21		4·4	
9 ,,	8		1·8	
10 ,,	28		6·2	
11 ,,	2	149	0·4	32·9
12 ,,	12		2·6	
13 ,,	13		2·9	
14 ,,	66		14·6	
2 to 3 weeks		93		20·7
3 to 4 ,,		32		7·1
4 to 5 ,,		19		4·2
5 to 6 ,,		8		1·8
6 to 7 ,,		1		0·2
7 to 8 ,,		5		1·1
2 to 3 months		1		0·2
3 to 4 ,,		1		0·2
4 to 5 ,,		1		0·2
5 to 6 ,,				
Exceeding 6 months		2		0·4
Totals		452		100·0

This table shows, that out of 452 fatal cases, 28, or 6·2 per cent. deaths happened during the first three days, 16 on the fourth, and 18 on the fifth; making a total of 62, or 13·7 per cent. during the first five days; 24 on the sixth day, and 54 on the seventh; being an aggregate of 140, or 31·0 per cent., during the first week. During the second week, 149 deaths, or 32·9 per cent., occurred, in the following proportions on the different days—viz., 4·4 per cent. on the eighth, 1·8 per cent. on the ninth, 6·2 per cent. on the tenth, 0·4 per cent. on the eleventh, 2·6 per cent. on the twelfth, 2·9 per cent. on the thirteenth, and 14·6 per cent. on the fourteenth day. We thus see, that of these 452 cases, 63·9 *per cent. died during the first fortnight.* Of the remaining 36·1 per cent., 20·7 were fatal during the third week, making the sum of 84·6 *deaths per cent.* in the course of the *first three weeks.* It also shows that 7·1 per cent. deaths happened in the fourth week, 4·2 per cent. in the fifth week, 1·8 per cent. in the sixth week, 0·2 per cent. in the seventh week, 1·1 per cent. in the eighth week, and only 1·0 per cent. subsequently. The table also affords the elements for calculating the average duration of the disease. Thus by multiplying the duration in days by

the number of deaths which occurred on each day, and then dividing the sum by the total number of deaths, taking the average for the second and third, and third and fourth weeks, &c., on the eighteenth and twenty-fifth days, we arrive at the conclusion, that the average duration of the disease in 447 cases of the total 452 cases, in which death happened before the expiration of two months, was 13·9 days; and of the total 452, allowing a duration of seven months each for the two cases whose duration exceeded six months, was 15·3 days.

These tables may be of much use in forming a prognosis: thus, if a child have survived a fortnight, it is more likely to recover than it was on the first day, in the proportion of more than two to one; if three weeks, of more than four to five, &c.

From the foregoing investigation, we are entitled to draw the following inferences: (a) *that nearly one-third of all the fatal cases of scarlatinal dropsy may be expected to die within the first week of the disease;* (b) *that considerably above one-half* (*say* 63 *per cent.*) *may be expected not to survive the first fortnight;* (c) *that the average duration of acute cases* (*i. e., those which are fatal in less than a month*) *is* 12·0 *days; and of all cases,* 15·3 *days; and lastly* (d), *that the particular days on which the disease is most fatal are the seventh and fourteenth, no less than* 54 *cases, out of* 452, *having been registered as fatal in the former, and* 66 *out of the same number on the latter day.*

The next subject for our consideration is *the duration of the disease in males and females, and in the different seasons of the year.*

It having been already shown (Article I., pages 234-6), that the mortality from scarlatinal dropsy is much greater in males than in females, and also, that it is in excess in the quarters ending September 30th and December 31st respectively, I have compiled the following tables to ascertain the influence of sex, and the seasons of the year, if any, on the duration of the disease.

TABLE XVIII.—*Scarlatinal Anasarca. Duration before Death. Males and Females.* 399 *Cases.*

Duration.	No. of deaths: Males.		No. of deaths: Females.		Deaths: Males and Females.
	Total.	Per cent.	Total.	Per cent.	Per cent.
1 to 2 days	8	3·5	4	2·7	3·0
3 to 4 „	17	6·7	7	4·8	6·0
5 to 6 „	23	9·1	13	8·8	9·0
7 „	31	12·3	17	11·6	12·0
	=79	=31·6	=41	=27·9	=30·0
8 to 9 „	16	6·3	11	7·5	6·8
10 to 11 „	23	9·1	4	2·7	6·8
12 to 13 „	11	4·4	9	6·1	5·5
14 „	33	13·1	25	17·0	14·5
	=83	=32·9	=49	=33·3	=33·2
2 to 3 weeks	54	21·4	38	25·8	23·0
3 to 4 „	13	5·1	11	7·5	6·0
4 to 5 „	10	3·9	5	3·4	3·8
Exceeding 5	13	5·1	3	2·1	4·0
Total	252	100·0	147	100·0	100·0

Before proceeding to consider this table, I would observe, that it is formed from a smaller number of cases than the preceding, in consequence of my having omitted to register the sex in conjunction with the duration, in the early extracts made from the returns. But it serves to show that the results are to be depended on, as the variations are comparatively small, although this table was compiled from the returns for the three last quarters of the year 1848, whilst the other was formed from the returns for the whole year. But there is also another reason for the variations in the number of deaths in the different tables—viz., that many of the returns were incomplete, some containing only the duration of the original disease, others the duration of the complications only; and in very many, it was altogether omitted.

The total number of males was 252, and of females, 147; being in the proportion of 63·1 per cent. males to 36·9 per cent. females. Of these respective numbers—viz., 252 males, and 147 females, 3·5 per cent. of the males, and 2·7 per cent. of the females, died on the first and second days of the attack; 6·7 per cent. males, to 4·8 per cent. females, on the third and fourth days; 9·1 per cent. males, to 8·8 per cent. females, on the fifth and sixth days; and 12·3 per cent. males, to 11·6 per cent. females, on the seventh day; making an aggregate of 31·6 deaths per cent. of the males, to 27·9 per cent. of the females, during the first week of the disease. During the second week, these proportions are, with one exception, reversed; for we find that 6·3 per cent. of the males died on the eighth and ninth days, to 7·5 per cent. of the females; 9·1* per cent. of males, to 2·7 per cent. of females, on the tenth and eleventh days; 4·4 per cent. of males, to 6·1 per cent. of females, on the twelfth and thirteenth days; and 13·1 per cent. males, to 17·0 per cent. of the females, on the fourteenth day. During the next fortnight, the proportion of deaths of females exceeded that of the males; thus, during the third week of the disease, 21·4 per cent. of the males died, to 25·8 per cent. of the females; and during the fourth week, 5·1 per cent. of the males, to 7·5 per cent. of the females. Subsequently to the fourth week, the proportion of deaths of males was in excess to that of females, in the ratio of 9·0 per cent. to 6·5 per cent. A comparison of the per centages of the males and the females respectively, with those of both conjointly, lead to similar results.

The conclusions which we draw from these considerations are, (a) *that, as before stated, a considerably larger number of males die from scarlatinal dropsy than of females;* (b) *that the second is the most fatal weekly period both for males and females;* (c) *that in proportion to the total number of deaths of each sex, a far larger number of males die on each day of the first week than of females;* (d) *that with the exception of the tenth and eleventh days, a large per centage of the females die during the second, third, and fourth weeks, than of the males;* and (e) *lastly, that of those who survive the fourth week, a greater ratio of males die than of females.*

I will present these conclusions in a tabular form, showing the per centage of deaths of 100 males and 100 females at these periods.

* I suspect some accidental circumstances produced this great difference.

	Males.		Females.
First week	31·6	...	27·9
Second, third, and fourth weeks	59·4	...	66·6
Exceeding four weeks	9·0	...	5·5
	100·0		100·0

If, however, these proportions are multiplied by the number of males and of females respectively who die from the disease, we should have a different, but an erroneous, conclusion, as the object of this was to show, not the absolute, but the proportionate, number of males or of females who die in each day.

The practical results to be drawn from these considerations are (*a*), that during the first week, we should expect the death of our male patients in a greater ratio than the female, and *vice versâ* in the second; (*b*) that if the patient survived the second week of the dropsy, he would have more than twice as good a chance of recovery as on the day of outbreak; and lastly, these tables teach that we should take especial care during the second week of scarlatina, to prevent exposure to cold air, or to any other agent likely to cause suppression of the cutaneous functions: and we ought always to have before us the fact, that nearly 64 per cent. of these deaths happened during the first fortnight of the dropsy. These tables also prove the disease to be of a far more acute character than is usually believed; and it is further to be remembered that the disease not only attacks males more frequently than females, and is more fatal to them, but that more males die during the first week than females. I am unable to offer any explanation of these peculiarities; indeed, until these tables were formed, I was not aware of their existence, although I have for years paid especial attention to this disease.

It having been shown that certain, and probably appreciable, atmospheric changes act unfavourably on the mortality from scarlatinal dropsy, it is important to ascertain whether this influence extends also to the duration of the disease; in other words, *whether the intensity, as well as the frequency of the disease varies in the different seasons of the year.* It has been already proved that the mortality from scarlatinal dropsy is in excess during the last six months of the year, August excepted, not only in proportion to the increase of scarlet fever, but also in a greater ratio. From these facts we should infer the intensity of the disease to be increased, and its duration to be proportionably diminished. The following table will elucidate these points:

TABLE XIX.—*Scarlatinal Anasarca. Duration of the Disease in each Season.*

Duration.	1st Quarter.		2nd Quarter.		3rd Quarter.		4th Quarter.	
Weeks.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.	Total.	Per cent.
1	16	31·4	20	39·2	46	33·3	58	29·2
2	14	27·4	13	25·5	53	38·4	67	33·7
3	8	15·7	11	21·5	20	14·5	47	23·6
4	7	13·7	2	3·9	8	5·8	12	6·0
5	1	2·0	3	5·9	6	4·3	6	3·0
6	1	2·0	1	2·0	3	2·2	4	2·0
7	4	7·8	1	2·0	2	1·5	5	2·5
Total	51	100·0	51	100·0	138	100·0	199	100·0

An examination of these data shows that the largest proportion of these 439 cases died in the second quarter during *the first week* of the disease; that the next largest number died in the third quarter; the third largest in the first quarter; and the smallest proportion in the fourth. During the *second week* of the disease, these proportions were much altered, the per-centage being greatest in the third quarter, and smallest in the second; the fourth and first having an intermediate mortality. The per-centage of deaths in the *first fortnight* was as follows: 71·7 per cent. in the third quarter, 64·7 per cent. in the second quarter, 62·9 per cent. in the fourth quarter, and only 58·8 per cent. in the first quarter. The ratio of deaths for each quarter during the *third and fourth weeks* of the disease was—29·6 per cent. in the fourth, 29·4 per cent. in the first, 25·4 per cent. in the second, and 20·3 per cent. in the third quarter. And of those deaths which occurred at a period *later than the fourth week*, the ratios were—11·8 per cent. in the first quarter, 9·9 per cent. in the second, 8·0 per cent. in the third quarter, and 7·5 per cent. in the last. The proportion of cases whose duration exceeded four weeks was greater in the first quarter than it would have been, from many of those who contracted the parent disease in September and October, when it is most prevalent, living through the quarter in which the dropsy first made its appearance.

Taking the duration of a fortnight as our standard, the above indicates the disease to be most rapidly fatal in the quarter ending September 30, *and least so in that ending March* 31, *which, taken in connexion with the mortality during the first four weeks of the disease, in the second and fourth quarters, will justify us in assuming that the disease is not only more prevalent during the last six months of the year, but also rages with increased intensity, and that it is least fatal in the first quarter:* but we must also remember that, whilst the mortality reaches its maximum in the quarter ending December 31, *the intensity* (*as evidenced by diminished duration*) reaches its culminating point in that ending September 30. In drawing these conclusions, I would observe, that they must be considered as approximative only, for they are deduced from an examination of four quarters only, and therefore subject to trifling errors. But to render a table of this kind absolutely certain, the data should extend over a series of years, which

would involve a work of very great magnitude, as there are no data ready at hand at Somerset House, and they would have to be obtained, as these were, by a separate examination of each return for the years examined. To form the table just considered, 57,771 returns were examined.

The proportion of deaths from scarlatinal anasarca *in males to females* has been already fully discussed, and need not be reinvestigated.* In the last half of the year 1848, 478 cases of anasarca occurred, of which 290 were males and 188 females, or in the ratio of 60·7 per cent. males to 39·3 per cent. females, being almost in the same proportion with the deaths from scarlatinal dropsy of all kinds.

The ages at which scarlatinal anasarca is most fatal are the same as those from scarlatinal dropsy of all kinds; and the same statement applies to the two sexes. A comparison of the following table with Table XVIII. conclusively shows this:

TABLE XX.—*Scarlatinal Anasarca. Mortality. Second half-year* 1848.

Age.	Males.	Females.	Total.	Per cent.
$\frac{0}{1}$	2	4	6	1·3
1	33	9	32	6·7
2	38	28	66	18·8
3	55	30	85	17·8
4	49	22	71	14·9
				= 54·5
5	38	21	59	12·3
6	25	22	47	9·8
7	24	20	44	9·2
8	14	13	27	5·7
9	10	7	17	3·6
				= 40·6
10	5	6	11	2·3
11	3	1	4	0·8
12	1	1	2	0·4
13	...	1	1	0·2
14	...	...	...	...
				= 3·7
15 to 20	2	1	3	0·6
20 to 30	1	2	3	0·6
30 to 40	...	...	...	...
Total	290	188	478	100·0

We here see, that of the 478 deaths from scarlatinal anasarca which were registered in the metropolis during the last half of the year 1848, 6 or 1·3 per cent. were of children under one year of age; 32 or 6·7 per cent. of children in the *second* year of their age; 66 or 13·8 per cent. in the *third* year; 85 or 17·8 per cent. in the *fourth* year; 71 or 14·9 per cent. in the *fifth* year; making a total of 260 or *of 54·5 per cent. in children under five years of age.* Fifty-nine cases or 12·3 were fatal during the *sixth* year of age; 47 or 9·8 per cent. during the *seventh* year; 44 or 9 2 per cent. during the *eighth* year; 27 or 5·7 per cent. during the *ninth*

* See Art. I., pp. 234—6.

year; and 17 or 3·6 per cent. during the *tenth* year; making an aggregate of 454 cases of the 478, or 95·1 *per cent. in children under ten years of age.* The remaining 4·9 per cent. occurred in persons above the age of ten years, and of these 4·9 per cent., 3·7 happened between the ages of ten and fifteen, leaving only 1·2 per cent. of deaths above this age. But we must not take these numbers for those who died above ten years as an average, for Table XIV., already so fully considered, and which extends over a period of five years, shows these to be somewhat erroneous, as being founded on too limited a number, and for too limited a period; but this table affords evidence of the truth, or a near approximation thereto, of the results for the ages under ten years. On comparing the deaths at the different ages from scarlatinal anasarca with those from scarlet fever itself, we shall find that the parent disease is most fatal by far in the first quinquennial period, both absolutely and comparatively; whilst the deaths from the anasarca are larger, in proportion to the fever, during the second quinquennial period. These results may be presented at a glance:

TABLE XXI.

	Under 5 years.		Between 5 and 10 years.		Above 10 years.		
Deaths from scarlatina	60·0	...	23·6	...	7·4	=	100·0
,, scarlatinal anasarca	54·5	...	40·6	...	4·9	=	100·0
,, scarlatinal dropsy of all kinds	53·1	...	40·1	...	6·8	=	100·0

As this statistical part has extended over so large a space, and involved a consideration of so many points, it will perhaps be useful to present the most important results as a whole, in as few words as possible.

(*a*) That the dropsy varies in fatality in the different quarters of the year, being most fatal in the fourth quarter, and, when compared with scarlatina, least fatal in the third, and most fatal in the fourth, quarter; (*b*) that the mortality in the third quarter varies greatly, one month—September—being that in which it is very fatal, whilst another—August—is the one in which it is least fatal in the whole year; (*c*) that the temperature between 42·0° and 52·0° corresponds with the most fatal period; (*d*) that the electrical state of the air modifies its progress more than any other atmospheric agent—as, when it was passive, the comparative mortality averaged 10·1, and when it was active, only 7·1; and (*e*) that the average of deaths was less in the wet than in the dry months. (*f*) That sex is one of the most powerful of all the modifying circumstances—for, of 946 deaths from the dropsy, 60·3 per cent. were males to 39·7 per cent. of females; (*g*) and that these proportions vary in the different quarters, the largest number of males dying in the fourth quarter, and the smallest in the second; and the largest number of females in the second, and the smallest in the fourth. (*h*) That age also modifies it to a very great extent, the largest number of deaths from the dropsy occurring during the fourth year, (*i*) that in proportion to scarlet fever, the dropsy is most fatal in the period between five and ten years of age; and (*j*), that in proportion to deaths from all causes, it is also most fatal in the same period. (*k*) That the causes influencing the fatality of scarlet fever affect that of the dropsy in a different ratio. (*l*) That the dropsy sets in most commonly on the fourteenth day; next in order, on the twenty-first; next, on the twelfth; next, on the seventh; and, lastly, on the thirteenth, eighteenth, twentieth,

and remaining days. (*m*) That the largest number die on the fourteenth day, the next largest on the seventh, and the next on the tenth; (*n*) and that of the weekly periods, the second after its invasion is that in which it is most fatal, next the first, then the third, then the fourth, and (*o*) that only 36·1 per cent. of the fatal cases survived the first fortnight, only 15·4 per cent. the first three weeks, and only 8·3 per cent. the first month; and that the average duration of the disease in the year 1848 was 15·3 days; and (*p*) that, therefore, the malady must be classed amongst the acute diseases. (*q*) That, proportionately to the females, most males die on each day of the first week; (*r*) that, on the other hand, except on the tenth and eleventh days, most females died on each day of the second, third, and fourth weeks, and (*s*) most males subsequently. And (*t*), that the duration varies in the different quarters, the disease being most rapidly fatal in the third quarter, and least so in the first; and, lastly (*u*), that the disease rages with greatest intensity during the last six months of the year, August perhaps excepted.

The great amount of space occupied by the statistical part of the paper, renders brevity in the remainder most necessary; wherefore a mere outline will be given of those divisions of the subject which have recently been most ably discussed in the previous volumes of this review. We refer especially to the articles on the works of Frerichs and Johnson. We shall, therefore, proceed at once to a very brief consideration of the *morbid anatomy of the kidney*. The extent and kind of the morbid changes vary according to the duration of the disease, and are those usually described as the first or second stages of Bright's disease. In the early stage the gland is heavy, large, and gorged to a greater or less extent with venous blood; its surface is usually smooth and of a dark colour, and intermixed with a greater or less number of points of a darker colour, and its capsule is easily removed. These dark spots, which are not always present, are caused by extravasation of blood into the tubules of the kidneys from rupture of the Malpighian vessels. On incising the organ, a greater or less quantity of a sanious fluid exudes, which contains blood corpuscles, epithelial cells, and fibrinous casts of the renal tubules. On examining the cut surfaces, we find the cortical substance much thicker than usual, of a dark colour, dotted over here and there with bright or dusky-red spots; the pyramidal portions are also congested, and often of a purplish colour, either entirely, or only of those parts adjacent to the cortical. The glands are also sometimes, even within a few weeks after the commencement of the dropsy, enlarged, and of a yellowish or mottled appearance externally, and present, when cut, the well known characters of the second stage of Bright's disease. On carefully examining those organs which are dark coloured, we sometimes find clots in their substance, caused by rupture of the intertubular plexus of veins. The mucous membrane lining the calices and pelvis is often congested, but sometimes unaltered. On making a microscopic examination, we find the outline of the renal tubules somewhat obscure and irregular, and themselves more or less filled with detached epithelium, either loose, or bound together by fibrine, forming the casts of Simon, or, as they are called by Johnson, epithelial casts. The quantity of epithe-

lium contained in the tubes varies very greatly, being sometimes so great as completely to fill its free canal, and thus press on the adjacent inter-tubular plexus of veins and impede the circulation. As before mentioned, we often find blood corpuscles in large numbers in the renal tubules, either free, or entangled in effused fibrine; indeed, in a bad case we almost always find blood corpuscles in the casts. The bright red spots seen on the incised surface are found, on microscopic examination, to be the congested vessels of the Malpighian bodies seen through the capsule, which is rendered opaque by the effused fibrine; or they may appear red without any change in the capsule, from the blood in the vessels being red instead of purplish. The epithelium normally lining the inner wall of the tubules, when present, appears of its ordinary shape, but is "sometimes unnaturally opaque and granular in texture" (Johnson), and is frequently more or less deficient; and in the advanced stage is, in many tubules, entirely wanting. When the course of the disease is favourable, the organs gradually assume their normal character, and but few, if any, of these morbid changes or deposits remain; whilst in severe cases, as in those which have become chronic, some of the renal tubules remain permanently deprived of their epithelium, and are consequently unable to perform their proper secretory function.

It may not be out of place here to mention that some doubts have been expressed, in an admirable article by Dr. Johnson's reviewer, as to the existence of a disease to which the name of "desquamative nephritis" is applicable. Frerichs also denies its existence. The reviewer admits that there is a disease in which "large quantities of renal epithelium are detached from the basement membrane of the tubes, and that from the loss of this epithelium evils of a serious nature result;" but he denies that sufficient proof has been afforded that this action is set up by the presence in the blood of a *mat ries morbi.* He does not consider it proved that this desquamation differs from what is observed in other organs, in which epithelium is detached from the basement membrane by fluid thrown out from the vessels subjacent to it. He adduces the desquamation of the bronchial mucous membrane in cases of bronchitis, in which no *materies morbi* is received into the blood, but in which the epithelium is detached by fluid loosening the "adhesion between the membrane and its scales." It seems, however, a matter of very little consequence whether the renal epithelial cells are detached from the membrane, by an affinity existing between them and a *materies morbi* circulating in the blood, or from fluid thrown out under the influence of this *materies morbi* from the adjacent vessels, by which their attachment to the basement membrane is loosened. For in either case, the desquamation appears to be, as regards scarlet fever, the result of the reception of the morbid poison into the blood, and its subsequent action on the kidney.

As our available space will not admit of discussing this point, we shall only submit the following for the consideration of our readers:—(*a*) That the peculiar phenomena grouped together under the term "scarlet fever," are induced by the absorption of a peculiar morbid poison into the system. (*b*) That in nearly all cases of scarlet fever, we can detect at some period or other of the disease, greater or lesser indications of a disordered func-

tion or diseased action of the kidneys. (*c*) That the evidences of disordered function or diseased action are, the presence in the urine, for an uncertain duration, of albumen and renal epithelium; and in some cases of blood corpuscles and other abnormal constituents. (*d*) That abnormities of the urine, similar to those present for a short period, and in small quantity, in most cases of scarlatina, exist in a greatly increased amount; and for a longer period in most cases of scarlatinal anasarca. (*e*) That renal epithelial cells cannot be detected in healthy urine; but (*f*) that they may be detected either entire or in fragments, whenever any substance which acts on the kidneys is exhibited for a few days consecutively. (g) *That large quantities of renal epithelium are, in some cases, present in the urine during the whole course of the disease, whilst fibrinous casts, blood-corpuscles, and other abnormal constituents are almost or entirely absent.* This is most commonly met with in the convalescent stage of scarlet fever; but sometimes in scarlatinal dropsy. (*h*) That several renal epithelial cells may be thrown off united together, and be found in the urine unaltered. (I have seen this in very many instances, but never unless associated, in the same specimen of urine, with blood-corpuscles and fibrinous casts.) (*i*) That in many cases of scarlatina, the post mortem appearances of the kidneys resemble those discovered in cases of scarlatinal dropsy.

As I shall have no other opportunity of alluding to the pathology of the disease, I will state my opinion, that the renal disease is the result of a specific action of the scarlatinal poison. I believe that the poison acts on the kidney in the same way as it acts on the skin and throat; and that the amount of its action varies in different epidemics, being slight in some, and intense in others. We have an analogous instance in its varying actions on the faucial mucous membrane. This opinion is the result of many years' experience, and of great opportunity of studying the disease; and I may mention that, during and since the year 1848, I have examined the urine of above 200 persons suffering from scarlatina and scarlatinal dropsy, and have almost invariably detected albumen, and evidences of desquamation of the renal epithelium at some period or other of both diseases. The exceptional cases have not amounted to five per cent. This theory also receives some support from the fact so well known to the profession, that scarlatinal dropsy occurs chiefly in those who have had the cutaneous and throat affection in but a slight degree.

(To be continued.)

Art. III.

On some Points in the Abnormal Anatomy of the Arm. By John Struthers, F.R.C.S., Lecturer on Anatomy, Edinburgh.

(Concluded from No. 26, p. 533.)

Relation of Supra-condyloid Process to Muscles and Region.—The anatomy of the soft parts in the neighbourhood of a supra-condyloid process appears to be the same, whether the nerve is accompanied by the artery or not. Proceeding from the tip of the process is a ligament or fibrous band, which arches downwards and inwards, and, blending with the intermuscular

septum, is inserted into the ridge a short distance above the condyle.* The true intermuscular septum dips down to be attached to the internal condyloid ridge of the humerus, and between this on the inside and the brachialis anticus and supra-condyloid process externally, is a grooved space, in which the deviating nerve and artery are placed. This hollow is bounded behind by the humerus, on which there is a more or less marked groove, the artery or nerve not lying actually in this groove, but in front of it. The inner boundary of the groove is the internal condyloid ridge; the outer gives origin to the most internal fibres of the brachialis anticus muscle. The nerve and artery begin to deviate at the tendon of the coraco-brachialis muscle, and pass down in this grooved space in front of the intermuscular septum, the nerve internal to the artery, and bound down by an aponeurosis from it to the brachialis anticus. They now lie between the intermuscular septum and the process, and passing underneath the concavity of the latter, are covered by it and protected from pressure. Their direction is now changed, and they pass obliquely outwards to gain their normal position at the elbow. From the point where they enter underneath the arch they may be covered by a high pronator teres, or by a strong aponeurosis passing inwards from a raised portion of the brachialis anticus. A high origin to the pronator teres, which may at first form a separate muscle, appears to be frequently if not generally present in cases of supra-condyloid process.

* *Internal intermuscular septum.* It appears to me that the normal anatomy of the part so named is not usually fully described. Reaching down on the inside of the arm, and connected with the aponeurosis, is a white cord-like band, but this is not the intermuscular septum. The true intermuscular septum lies some distance in front of this, and is not so evident on the surface of the muscles. The true septum dips down as a strong fibrous membrane, to be attached to the internal condyloid ridge, and is fully half an inch in depth, forming a fibrous partition between the brachialis anticus in front, and the inner head of the triceps behind, to both of which it gives origin. Below, it passes to the internal condyle, having before been joined by the posterior ligamentous band; and above, it is continued along the internal border of the humerus, as far as the insertion of teres major, having again been joined by the posterior ligamentous cord. The latter separates from the true septum at or above the insertion of the coraco-brachialis muscle, and passes down on the surface of the internal head of the triceps, lying posterior to the true septum, at a distance varying from three-fourths to a fourth of an inch, and again it joins the true septum about an inch, more or less, above the condyle. It is connected in front to the true septum by an aponeurosis, through which the muscular fibres are visible; and behind it is continued into the aponeurosis of the posterior brachial region. Close behind it is the ulnar nerve and inferior profunda artery. I have seen the nerve lie some distance behind it, and uncovered by it; but, usually, the nerve will be exposed by an incision along the posterior edge of this fibrous cord. It appears to have reference to the position of the nerve, and, forming a tolerably resisting band over or along the hollow on the inside of the humerus in the lower half of the arm, may serve to protect the ulnar nerve from any pressure against the inside of the arm. It is united above to the true intermuscular septum, the two together forming a fibrous cord, running up along, and attached to, the internal border of the humerus, and passing behind the tendon of the coraco-brachialis. The latter is usually said to be intimately connected at its insertion with the intermuscular septum. It appears so, and on one or two occasions, I have found some fibres of the tendon continued into the septum; but usually, they may be easily separated down to the bone; and it is evident that the septum passes up behind the tendon, as far up as the teres major, or as high as the highest point of origin of the internal head of the triceps, of which it appears almost like a tendon of origin in its whole length. I have repeatedly examined these points with care, dissecting down to the bone, and the above will be found almost invariably to be the precise arrangement. I shall again refer to the deceptive feeling, as if the true intermuscular septum was attached not to, but in front of, the internal condyloid line. We must, therefore, distinguish the true intermuscular septum, dipping down to the bone, the edge of which is not very apparent at first on the surface, from the very evident ligamentous cord behind it, which usually bears a close relation to the ulnar nerve, and might be called the *internal brachial ligament*, as a means of distinguishing it from the true septum.

Relation of Supra-Condyloid Process to Humerus.—It has been already mentioned that the supra-condyloid process is situated, with very little variation, at the distance of two inches above the upper edge of the internal condyle. Its relation to the surface and borders of the humerus is also constant. It is always placed on the internal surface, either midway between the internal and anterior borders of the bone, or a little nearer to the latter; and the ligament which completes the arch has to pass downwards and inwards to join the intermuscular septum above the condyle. This constancy of the position of the process, both as regards the distance above the elbow and the surface of the bone, is remarkable and interesting; and, both in position and direction, the process will be found to correspond closely to the arch of bone by which the foramen is completed in the lower animals.

I must here remark upon a method of describing the shaft of the humerus to be found in several of our best text-books of anatomy, by which this bone is represented as presenting two surfaces only, an anterior and posterior. But the humerus, like the other long bones of the limbs, has distinctly three surfaces and three borders. The error as regards the humerus, has no doubt arisen from the circumstance that two of the surfaces may be seen on the anterior aspect, but these two are no less distinct from each other than they are from the posterior. It is only in the lower fourth of the shaft that it becomes flattened from before backwards, and from this part up to the surgical neck, the shaft is at least as thick on a side view as when s en in front; and transverse sections of the bone are at all parts of the shaft more or less triangular. The lateral borders are continued up from the condyloid lines, the outer ending at the back part of the greater tuberosity, whilst the inner is lost at the surgical neck, after passing up, faintly, a short distance behind the inner edge of the bicipital groove. The anterior border is the most distinct of the three, except in the lower fourth, where, however, it is still quite distinct, though not sharp or rough. It is formed in the upper third by the external bicipital ridge, and is continued down from it to within half an inch of the coronoid fossa, which it bifurcates to enclose, and separates the external from the internal surface, the former in the lower half being the broader of the two, owing to the prominence of the external condyloid ridge.

The internal surface is that which I desire more particularly to notice. It supports the supra-condyloid process in the situation already defined; and behind the process, or in this situation, there is a groove. This groove has been mentioned by Dr. Knox in his papers already referred to, as existing in many arm bones. This part of the internal surface of the humerus is generally somewhat concave, though sometimes it is flat or even a little convex, and I certainly have found this groove present in the majority of a large number of arm bones that I have examined on purpose. In some this groove is altogether wanting, in a considerable proportion it is well marked, and in the majority it is present. It is seen in some young and adolescent bones, and appears to be at least equally distinct in those bones, the smoothness and straightness of which indicate them in all probability to have belonged to female skeletons.* This

* There is no separate bone in which the difference between the male and female (or at least the muscular and less muscular) is usually more striking than the humerus. In articu-

supra-condyloid groove is bounded posteriorly by the internal border or condyloid ridge, and in front by a special ridge, which marks off the posterior half or third of the internal surface as the groove. It reaches for about an inch down from the situation which a supra-condyloid process would occupy, and from one to two inches upwards from it, to near the usual situation of the principal nutritious foramen.

Taken along with the existence of a supra-condyloid process, this groove is naturally supposed to correspond to the position of the deviating nerve or artery; but in two of my specimens with this process, the groove is not so distinct as in the opposite bone, where there is no process and when there was no deviation of the nerve or artery; and in case No. 3, the process existing on both sides with deviation of both nerve and artery, the groove cannot be said to exist. Besides, the occurrence of the deviation of the nerve and artery, or of a supra-condyloid process proper, is comparatively rare—whilst I have said this groove exists in the majority of arm bones; and farther, since the artery or nerve does not properly occupy or lie in it, but only in front of it, even when the deviation exists, we must conclude, that the groove is not there for the purpose of lodging a vessel or a nerve. Being much puzzled to explain the meaning of this groove, I have lately carefully examined in a great many arms the exact relation of the soft parts to the humerus at this part, and the following will be found to be almost invariably the exact arrangement.

The true intermuscular septum gives attachment by its posterior surface to fleshy fibres of the inner head of the triceps. On dissecting these fibres off down to the bone, the septum is felt to be attached to a sharp ridge, and a little distance behind this ridge, a prominent but somewhat rounded edge of bone is felt, as if it was the internal border of the humerus. It appears as if the septum were not attached to the border, but to a ridge one-sixth or one-fifth of an inch in front of it; but this, however plainly felt by the finger in the dissection, is deceptive. The bone is at this time partially rotated outwards, so as to turn its posterior surface a little forwards. On careful examination of the macerated bones, the condyloid ridge is seen to be the true internal border when the humerus is fairly looked at in front, but if turned a little outwards, the part of the bone behind the condyloid line now projects. The true intermuscular septum, then, is really attached to the internal condyloid ridge or true internal border. On now dissecting on the outer or anterior aspect of the septum, it is seen to give origin to fleshy fibres of the brachialis anticus, down to the bone. The brachialis is now seen to arise directly from the surface or floor of the groove, also by fleshy fibres, and on separating these the outer edge of the groove is usually very distinctly felt by the finger, and is often especially rough at about two inches above the condyle. From this ridge, and especially at this rough part, the brachialis anticus arises usually by distinct tendinous bundles or fibres, whilst its origin is again fleshy to the outside of the ridge. The groove is usually very distinct to the touch when we have dissected down to it

lating four pairs of these humeri to each other, I noticed the curious fact, in three of them, that the right is longer than the left. In one pair, male, the difference is a quarter of an inch; in the other two, female, it is as much as half an inch. It would be interesting to ascertain whether the right humerus is generally thus longer than the left.

in this way, the distinct and sharp inner boundary giving attachment to the intermuscular septum, while the sometimes still more distinct outer boundary, at the part mentioned, is seen to give origin to the tendinous bundles for the brachialis anticus. This groove, then, generally existing on the humerus, has no relation to the position of a nerve or artery. It appears not to be constructed to serve any purpose, as a groove, but to result from the development of the two ridges, the internal giving attachment to the intermuscular septum, whilst the meaning of the outer and shorter one remains to be noticed. Considering its position on the internal surface of the humerus, considering that its roughest part is just two inches above the condyle, that a small point or ridge-like tubercle may exist on it here, and that this is precisely the point from which the supra-condyloid process in its various stages grows, it appears legitimate to draw the conclusion that this short rough ridge is a rudimentary condition of the supra-condyloid process, the base of which grows out from such a ridge running vertically on the bone at this part. This development into a process, however, but rarely occurs, and the *rudimentary ridge* is employed for the specific purpose of giving origin to tendinous fibres for the brachialis anticus. We have seen that, in every case in which a process existed, the median nerve, when its position was noted, deviated and passed round it, but we have other cases still to record in which the nerve and artery deviated, but in which there was no process beyond the existence of the short rough ridge. The correct view to take, perhaps, is this —that the median nerve, accompanied or not by the artery, occasionally deviates, and that, when it does so, we occasionally or usually find a protective arch thrown over it, formed by a supra-condyloid process above, and a ligament below, being an arrangement analogous to that which occurs in the most highly formed condition of this part of the mammalian humerus.

Comparative Anatomy.—Although there is no doubt that the foramen in the humerus affords protection to the parts which it transmits, it is not understood precisely in what manner it does so in the animals in which it exists, nor why they, more than some others, require such an arrangement for the protection of the vessel and nerve.

The arrangement is found among the Quadrumana, Rodentia, Edentata, Marsupialia, and more frequently among the Carnivora. The cat presents the most familiar example, and the following description is taken from my dissections of the part in that animal.

Bone.—The humerus is three inches and a quarter in length, and the situation of the upper end of the arch which completes the foramen, is half an inch above the internal condyle. The supra-condyloid process in man occupies the same proportional situation, being two inches above the condyle, whilst the humerus is, on an average, twelve inches in length; that is to say, the distance of the foramen in the cat, and the process in man, from the lower end, or internal condyle of the humerus, is about one-sixth of the length of that bone. The supra-condyloid foramen is a short oblique passage, directed downwards and forwards. The space is oval, measuring one-fifth of an inch in depth, and one-twelfth of an inch in breadth. It is formed on the internal aspect, by an arch of bone leaving and again joining the shaft. This arch, however, does not project

abruptly outwards, like the process in man, but only slightly bulges outwards the natural sweep from the condyle to the inner border of the humerus, and, in the adult bone, forms a lesser projection than that of the condyloid ridge on the outside. The foramen, therefore, is not, as some have supposed, merely a space left to hold and protect the artery in consequence of the great development of this part of the bone,* and by which the artery would otherwise have been thrown out of its course; and the development of the foramen farther shows that it is originally formed as a specific provision.

Development of the Foramen.—I find that the arch, by which the foramen is at length completed, grows, as a process, from above downwards. It is developed from the shaft, and again unites with the shaft below, and is completed altogether independent of the epiphyses of the lower end of the bone. This will be better understood by reference to the illustrations, figs. 4, 5, and 6, in which the bone is reduced one-third from the natural size. Figs. 4 and 5 are from the new-born kitten, the cartilaginous ends being preserved in the second. Besides a number of previous observa-

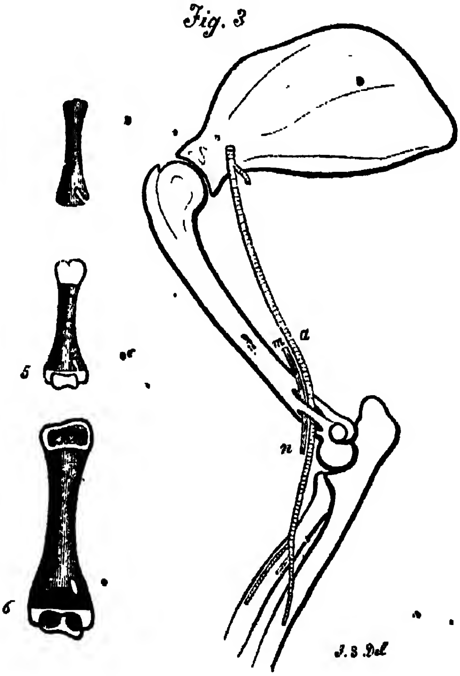

Fig. 3. Anterior extremity of cat, reduced one-half. *a*. Brachial artery. *m. n.* Median nerve.

Figs. 4 & 5. Humerus of kitten at birth, without and with the cartilaginous extremities.

Fig. 6. The same at the fifth week after birth; also showing the development of the supra-condyloid foramen.

* Introduction to the Natural History of Mammiferous Animals. By W. C. Linnæus Martin, p. 89.

tions, I recently examined both limbs in three new-born kittens of the same birth. In some the lower end of the process is not yet united to the shaft; in others it is just about to unite with it, so as to complete the bony foramen.

Fig. 6 is from the kitten, five weeks after birth. The foramen is seen to be completed as part of the shaft, and independent of the epiphyses, which are now ossifying.

The arch by which the foramen is completed, thus resembles, during the early stage of its development, the supra-condyloid process on the human arm; and were the latter prolonged in the direction of the ligament, it would correspond exactly to the arrangement in the cat, with this difference, however, that in man it starts more abruptly away from the humerus, although, on the other hand, its direction is not greater in this respect than is necessary to leave a sufficient space for the nerve and bloodvessels. Seeing this to be the mode of its development in animals, we are not surprised to find it, when it is present in man, to occur in the very young subject and female, as well as in the adult and muscular. It is not unlikely that the arch may be liable to variety in the extent of its development, leaving the foramen unfinished, in animals which normally have it completed. In one arm of an adult cat, I found the arch represented by a ligament only, which, both above and below, joined a short spiculum of bone. On the other side, the arch was bony and well formed, as seen in fig. 3, which is sketched from the preparation made from it, and now in my collection. In the adult bone, the arch is directed downwards and backwards towards the internal condyle, is flattened, measuring one-tenth to one-twelfth of an inch in breadth, is somewhat concave towards the foramen, and convex and smooth superficially. There is a distinct groove on the humerus behind the foramen, both above, where the nerve and artery lie, and below, where no nerve or vessel is near, but almost no groove, or a very short one, in front of the opening, which corresponds to the abrupt departure of the artery and nerve after they have traversed the foramen.

Nerve and Bloodvessels.—The brachial artery does not lie as in man, along the course of the humerus, but as it crosses the subscapularis muscle, is removed considerably to the inside of that bone. It now reaches obliquely downwards and outwards, supported behind by the internal head of the triceps, approaches and touches the humerus just before entering the foramen, through which it now passes along with the median nerve. It then leaves the humerus, lies at a considerable distance in front of the elbow, and, after a course of an inch and a quarter after leaving the foramen, it divides into the radial and ulnar arteries. The median nerve accompanies the brachial artery in the arm, through the foramen, and below it, always lying to its outside, so that the nerve lies highest as they traverse the opening. I have not noticed any veins passing through the opening with the artery, and in one dissection in which the veins were injected, they did not pass through, but those accompanying the arteries in the forearm, joined the superficial veins below the foramen, and, passing with them over the arch, reached up the arm superficial to the artery.

Muscles.—There is no deep muscle by which the nerve and artery would apparently be directly or unavoidably compressed against the bone,

unless by the muscle corresponding to the biceps of man, the inner edge of which somewhat overlaps them. This muscle will press back against the protective arch, but it is not evident that the nerve and artery might not simply have been pushed aside by its action; and in animals in which the foramen does not exist, as in the dog, this muscle has the same relation to them. In the cat, there is a superficial muscular expansion covering the whole of the internal brachial region, and of considerable thickness over the foramen; but, in the lion, this expansion has become aponeurotic before it reaches over the foramen. In the lion, I found the veins did not pass through the foramen; and in an ichneumon which I dissected lately, the nerve alone passed through on both sides, the artery and veins passing in front of the arch.

It may be inferred from this, that the foramen is primarily intended for the nerve, and this agrees with what we have seen from the cases of the variety in man, that, when the process exists, the nerve always deviates, whilst the artery may or may not. It may be compared in this respect to the supra-scapular notch in man, which always transmits the nerve, while the artery passes over the ligament and only occasionally under it, with the nerve. It is not unlikely that the nerve only may pass through in many of the animals, the skeletons of which present the supra-condyloid foramen; and also it is not unlikely that a process may occasionally be found as a variety on the humerus of animals which, like man, do not possess it as a regular character.

III. On the Varieties of the Arteries of the Arm.

The varieties affecting the arteries in the arm may be arranged in three classes:—1. Varieties in the muscular relations; 2. Deviation of the artery from its usual course; and, 3. An early division of the artery by which two or more arteries exist, instead of one.

1. The first of these classes of varieties I have above considered and furnished examples of, in the cases of the artery being covered by a slip from the latissimus dorsi, by an expansion from the coraco-brachialis, by a broad third head to the biceps, by an aponeurosis from the brachialis anticus, or by a thin portion of the muscle itself, and by a high origin of the pronator teres.

2. The second class is made up nearly altogether by the variety in connexion with a supra-condyloid process. In this, *the condyloid deviation*, the position of the artery is changed in the lower two-thirds of the arm. Accompanied, as usual, by its venæ comites and the median nerve, it is covered only by the aponeurosis and a sheath of fascia, derived from the outer edge of the intermuscular septum, along which it runs. But below the process, the artery, besides being directed obliquely outwards, is generally more deeply covered than usual, either by an aponeurosis from the brachialis anticus, or a high origin of the pronator teres.

As the artery, at the process, lies three-fourths of an inch from the inner edge of the biceps, the ordinary incision along the edge of that muscle could scarcely enable the surgeon to reach the artery, but an examination of the arm before the operation was begun would probably indicate the deviation of the artery by the unusual situation of the pulsation, and in the cases where the process is well formed it is easily felt from the surface.

But the artery may deviate in this manner independent of the existence of a supra-condyloid process; as in four cases described and figured by Mr. Quain. In these, the median nerve continued in the course of the intermuscular septum,—accompanied, in two of the cases, by the undivided brachial artery,* and in the other two by one of two trunks into which the brachial had divided,†—and crossed or perforated the intermuscular septum, two or three inches above the internal condyle, and then inclined outwards to the usual position, under cover of a high origin to the pronator teres muscle.

I have met this winter with a well-marked case of this condyloid deviation, without the co-existence of a developed supra-condyloid process. The median nerve and undivided brachial artery deviate and pass down in front of the intermuscular septum to within $1\frac{3}{4}$ inch of the condyle, and now pass beneath a fibrous arch and a high pronator, teres, and are directed downwards and outwards to the bend of the elbow. As the nerve passes down with the artery and its venæ comites, the nerve lying internal, they are sunk in a depression between the septum and the brachialis anticus. On dissecting aside the fibres of the brachialis anticus which lie behind and support the nerve and artery, a groove is distinctly felt in the humerus. The outer edge of the groove is a ridge, giving origin to fibres of the brachialis, and presenting a more elevated portion, like a rudimentary supra-condyloid process, exactly two inches above the condyle, and situated nearer the anterior than the internal border of the humerus. From this more prominent point arises the fibrous arch, which then stretches down to join the intermuscular septum. In this case, then, there is a true condyloid deviation, exactly as in the cases of a developed process, but here the process could scarcely be said to exist.

As above related, in thirteen of the cases in which an artery deviated, in connexion with a supra-condyloid process, it was the entire brachial artery in nine, and one of two large vessels in the remaining four. In the latter case the surgeon would be very apt to overlook one of the arteries, and I suspect it must have been the occurrence of some such variety which has led to the common observation, that the inferior profunda branch is liable to be mistaken for the brachial artery in an operation.

3. Regarding the high division of the brachial artery, the relative frequency of it, and the various nature of the early branches, little remains to be done after the careful and extended observations of Mr. Quain, as recorded in his beautiful and valuable work on the anatomy of the arteries. From the observation of 481 arms, he has shown that the proportion of cases in which two arteries exist in the arm is one in $5\frac{1}{5}$. The previous estimate of Harrison, given in his valuable treatise on the arteries,‡ from the observation of 82 cases, gave a proportion of about one in four. Adding together the observations of Harrison and Quain, the proportion will stand about, though rather less than, one in five. The early vessel is well known generally to be the radial, sometimes the ulnar, and occasionally a high interosseous; or, by a short union or cross branch below, a condition equivalent to a double brachial artery, with the two divisions either equal or with one of the vessels much smaller than the other.

* Op. Cit., p. 259. † Plate xxxvi. fig. 4; and Plate xxxvii. fig. 1.

‡ The Surgical Anatomy of the Arteries. Dublin, 1829.

But, although these details are of much interest to the anatomist, they in reality are of little importance to the surgeon, who is concerned with this fact only, that frequently there are two arteries instead of one, and that it may be so in the case in which he is about to operate. The important practical question then comes to be, supposing a second artery to be present, how is its existence to be ascertained, and how can it be reached?

My attention has been directed to observing the exact *relative position of the two trunks*, more than to ascertaining the relative frequency of the different varieties; and, although this opportunity is very apt to be lost in the ordinary dissections in the anatomical rooms, I have been able to note the exact position of the two vessels in twenty arms, eighteen of these presenting instances of the high radial, and the remaining two, of the high ulnar artery; and ten of these occurred in sixty subjects dissected during the earlier part of the present session. It would be tedious to give the details in each case from the notes now before me, and will suffice if I state the conclusions to which I have come. In nearly all of the cases the division occurred in the axilla or high in the arm, thus presenting two arteries throughout the whole or greater part of the brachial region. It is necessary to consider the upper and lower parts separately, taking the insertion of the coraco-brachialis, or where the artery normally crosses the lower end of that muscle, as the termination and commencement of the two portions.

In the first portion of their course, the two arteries lay, in all the cases, very near to each other, separated only by the median nerve and a layer of deep fascia or sheath. One of the trunks lies superficial to the other. It is the high radial or ulnar as the case may be, and is usually, more or less, the smaller of the two; the other and deeper trunk represents more the position of the brachial, the median nerve being related to it as it is normally to the single brachial artery. The high radial, I have said, is somewhat superficial in position to the other, but it may, at the same time, lie to either side, or directly over the deeper trunk. In most of the cases it lay external and superficial, but in some it arose on the inner or anterior aspects, and then crossed over the deeper trunk to get to its outside. This will be found to be the exact position of the two arteries in the *upper part* of the arm, one lies immediately below the aponeurosis, and superficial, or on a plane anterior to the other. Close either on its inner or outer side, and behind, or on a plane posterior to it, is the other trunk, the median nerve crossing very obliquely between the two, and a layer of sheath also intervening.

The deeper trunk, if the division is as high as the subscapularis muscle, furnishes the circumflex and subscapular, and then the profundæ branches, while the more superficial trunk, keeping down by the edge of the biceps, furnishes the external muscular branches.

In the *lower half* of the arm there is considerable variety in the relative position of the two trunks, and they seldom lie so close together as above described in the upper half. It is necessary here to distinguish between high radial and high ulnar. As Mr. Quain has remarked, the high ulnar has a tendency to pass to the inner or ulnar side away from the edge of the biceps. In both of my cases, the high ulnar was, below, removed half an inch from the inner margin of the biceps, the deeper trunk in one of the cases being external to it, in the other internal,

having the position of the condyloid deviation. In both, the high ulnar passed over the muscles of the forearm, although not superficial to the aponeurosis. In the more numerous and common cases of high radial, I have observed, that the more superficial of the two, or high radial, courses down along the inner edge of the biceps, covered by the aponeurosis and, it may be, slightly overlapped by the edge of the biceps, whilst the other trunk occupies a position internal to it and deeper, to a varying extent. The median nerve is at first still crossing obliquely between the two arteries, but, in the lower third, has now gained the inside of the deeper trunk and descends parallel to its inside, in the situation where the two arteries are most separated from each other. The deeper trunk is generally, if not always, covered by a second aponeurosis. It is very frequently, if not generally, lodged or sunk in a groove in the brachialis anticus muscle, and from the raised or projecting fibres on the outside of the artery, an aponeurosis is sent inwards over the artery and nerve, binding them down upon the surface of the brachialis muscle, or into a groove in it. Frequently also, the fleshy fibres themselves pass across or partly across, and bind down or overlap the artery and nerve, as already described to be a position not uncommonly occupied by the undivided brachial artery.

The deeper trunk is thus so placed, that, to expose it, requires the division of a deeper layer of aponeurosis, or the division or displacement of some muscular fibres, but a more important point is the frequent removal of the deeper trunk more or less to the inner side, gradually approaching the intermuscular septum, until it constitutes a condyloid deviation of the deeper of the two trunks.

In some of the cases the deeper artery lies close to the inside of the high radial, but deeper, in some it was a quarter of an inch to the inside, in some half, and in several three-fourths, of an inch; in either case bound down by the deeper layer of aponeurosis. In five of the cases, this inward separation went as far as to constitute the condyloid deviation. Two of these five were on opposite arms of the same subject, and in one of the subjects the non-deviating artery was the high ulnar. In the two occurring in the same subject, the anatomy was the same on both sides. The deviating trunk was three times as large as the non-deviating high radial. The deviation commenced at the insertion of the coraco-brachialis. The larger artery, with the median nerve lying in this case still to its outer side, passed down in front of the septum, sunk into a depression between it and a portion of the brachialis anticus muscle, to within 2½ inches of the condyle, when they inclined outwards to pass below a fibrous arch and a high pronator teres. Higher up the arm than the high pronator and fibrous arch, the deviating artery is bound down by some overlapping fibres of the brachialis anticus, and an aponeurosis is prolonged from these to join the intermuscular septum. Here the artery lies half an inch inwards from the high radial. At the bend of the elbow they are separated by the high pronator, over which, but underneath the semilunar tendon of the biceps, the high radial artery lies.

In the other three subjects, the anatomy of the deviating vessel was so exactly the same that one description will suffice. There was no high pronator teres, or fibrous arch proper. The artery left the high radial, or ulnar, at the insertion of the coraco-brachialis, and passed down in

front of the intermuscular septum, sunk into a deep depression between it and part of the brachialis anticus, supported behind by the fibres of the brachialis which arise from the septum. The median nerve has crossed over the deeper artery at the middle of the arm, and now passes down parallel to its inner side. Arrived at within 1½ inch from the condyle, the nerve and artery now turn gradually and incline outwards and downwards to the elbow. Before the artery thus begins to incline outwards, it is placed ½ to ¾ inch to the inside of the high radial, which lay along the edge of the biceps, but, in one of the three, the other artery, being the high ulnar, was nearer to it, having itself inclined inwards half an inch from the inner edge of the biceps. From the middle of the arm downward, in these, as in all cases of condyloid deviation, the artery and nerve, lying in the deep groove in front of the intermuscular septum, were bound down by a strong aponeurosis passing between the septum and raised fibres of the brachialis anticus, and, especially below, for two or three inches, also partially concealed by some of the fleshy fibres of that muscle.

The *practical conclusions* from these observations regarding the relative position of the two arteries, may be easily drawn. In a preliminary examination of the arm, the existence of two arteries may be ascertained by the pulsation, or it may be visible, as I have seen in an emaciated person. If no such preliminary information can be obtained, and if the artery first met with is not unusually small, and the ligature of it arrests the pulsation or bleeding below, there is no necessity or warrant for making a farther search. If it is otherwise, another vessel must be sought for. The artery first met with will, in all probability, be the more superficial. If it is in the upper half of the arm, the other artery will, if indeed the pulsation has not already indicated its position, be readily found a little deeper, separated only by the median nerve and a layer of fascia or sheath, either directly behind, or close to one side of, the one first found. The mere appearance of the artery being small, does not necessarily indicate that it is only one of two vessels, as, in some female subjects especially, the arteries look very small, and are, in all persons, considerably smaller than the distended arteries seen in the dissecting room and in museums; and, besides, it is not easy to judge of the true size of an artery in an operation, owing to the very limited extent to which it is exposed. It has been recommended, in the case of two arteries being met with, to secure that one only which is connected with the aneurism or bleeding vessel, but it is evident that there can be no greater danger to the vitality of the fingers from tying two arteries in a high division, than in tying the one artery when it is single, and various good reasons might be assigned for the rule to secure both. This rule may be departed from in the cases of false aneurism, and wound, when it is enough to place two ligatures on the artery concerned, one on each side of the aneurism or wound. These being the chief conditions requiring an operation in the lower part of the arm, it may not be necessary to search for the other artery, although the one first secured should evidently be only one of two. But if it is desired, the above anatomical details show that it will not usually be so easily found as in the upper half of the arm. It is often concealed by fleshy fibres from the brachialis anticus, and generally, either with or without this, bound

down upon that muscle by a deeper aponeurosis. The median nerve is not now over it, but to its inner side, and the artery and nerve are often more or less removed towards the intermuscular septum, sometimes for three quarters of an inch; so that the artery will be found, not only deeper, beneath muscular or aponeurotic fibres, but in some part of the space between the inner edge of the biceps and the intermuscular septum.

Art. IV.

Notice of some Eudiometric Researches. By Dr. G. Valentin, Professor of Physiology in the University of Bern. (Communicated by William Brinton, M.D., Joint Lecturer on Physiology at St. Thomas's Hospital.)

In a letter lately received from Professor Valentin, some important facts are mentioned respecting the influence which the prepared frog exerts on the atmospheric air surrounding it, while its excitability still continues, and it has disappeared either from putrefaction or from an artificial agency.

"1. In order that the irritability of a prepared frog, which remains enclosed in a space filled with atmospheric air, and saturated with watery vapour, should be preserved for days together, the skin ought not to be removed. The denuded muscles lose their excitability so soon, that many of these points cannot be accurately determined.

"2. Such preparations, clothed with their skin, consume oxygen, and give off carbonic acid, whether they are still irritable or not.

"3. On losing their excitability, they give off more carbonic acid in proportion to the oxygen absorbed. If the preparation be allowed to take on the ordinary process of putrefaction, we get in its first periods less carbonic acid and more oxygen, and subsequently, when all irritability has disappeared, a larger proportion of carbonic acid. But when a fresh preparation is deadened by cold or mechanical injury, this larger proportion of carbonic acid appears at once. An example may illustrate what I have been telling you: (*a*) Two irritable preparations, which had remained 315 minutes in about 1·41 cubic inches of atmospheric air, gave off during this time 1·06 per cent. of this volume of carbonic acid, and consumed 3·97 per cent. of oxygen; (*b*) After being deprived of their irritability through cold, they gave off, in 296 minutes' sojourn in about 1·436 cubic inches of air, 1·62 per cent. of carbonic acid, and consumed 2·72 per cent. of oxygen. So that we get the proportion of carbonic acid given off to the oxygen taken up, during the state of excitability, = 1 : 3·74; after its removal by cold, = 1 : 1·68.

"4. One mode of destroying irritability leads to a different result. When the preparation has been rendered unexcitable by exposure to a temperature of 122°—212° Fahr., it afterwards consumes much oxygen, but forms very little carbonic acid, so that the relative amount of carbonic acid becomes even less than in the irritable state.

"5. The proportions of the nitrogen which most impede the process of analysis, lead to just as decisive results. As long as the preparation retains its irritability, the nitrogen probably remains unchanged. I say *probably*, because the only differences in the calculation lie within the limits of errors of observation. While, on the other hand, when the irritability has been suppressed in any way, nitrogen is given off in quantities which exceed these limits of error.

"The more we follow these phenomena, the more shall we convince ourselves that the eudiometric changes are very delicate indications of the material interchange of the animal tissues. But these eudiometric analyses must be made with the greatest possible precision, and not carried out in the usual superficial manner."

PART FOURTH.

Chronicle of Medical Science.*

REPORT ON MATERIA MEDICA.

By EDWARD BALLARD, M.D.,
Physician to the Farringdon General Dispensary.

I. *On the Acidity, Sweetness, and Strength of Wine, Beer, and Spirits.* By H. BENCE JONES, M.D., F.R.S. (Phil. Mag, Feb. 1854, p. 143.)

1. THE *acidity* was determined by means of a standard solution of caustic soda. The quantity of liquid neutralized was equal in bulk to 1000 grains of water at 60° Fahr. The acidity is represented as follows:—In sherries it varied from 1·95 to 2·85 grains of caustic soda; Madeira, from 2·70 to 3·60; port, from 2·10 to 2·55; claret, from 2·55 to 3·45; Burgundy, from 2·55 to 4·05; Champagne, from 2·40 to 3·15; Rhine wine, from 3·15 to 3·60; Moselle, from 2·85 to 4·50; brandy, from 0·15 to 0·60; rum, from 0·15 to 0·30; Geneva, 0·07; whisky, 0·07; bitter

* The Reports included in the Chronicle are not intended as complete summaries of the subject. Still it is hoped that no important paper will escape notice. In the present number, the Medical Report and the Therapeutical Record have been omitted for want of room. Care will be taken, however, in future numbers to insert all the Reports with regularity.

During the months of March, April, and May, the following foreign journals were received:

GERMAN.

1. Archiv für Phys. Heilkunde, von Vierordt. 1854, Heft 2.
2. Archiv für Pathol. Anat., &c. von Virchow. Band vi. Heft 3.
3. Archiv für Anat., von J. Müller. 1854, Heft 1.
4. Zeitschrift für Rat. Med., von Henle und Pfeüffer. Band iv. (N. F.) Heft 2.
5. Zeitschrift der K. K. Gesell. der Aertze zu Wien, von Hebra. 1854, Feb., Marz, April.
6. Zeitschrift für Klin.-Med., von Ginsburg. Band v. Heft 1.
7. Zeitschrift (Henke's) für die Staatsarzneikunde, von Behrend. 1854, Heft 2.
8. Prag. Vierteljahrschrift für die Prak. Heilk. 1854, Band xxv, xxvi.
9. Verhandlungen der Phys.-Med. Gesell. in Würzburg. Band iv. Heft 2.
10. Verhandlungen der Gesell. für Geburtshilfe in Berlin. Heft vii.
11. Canstatt's Jahresbericht für Gesammten Medicin, in Jahre 1853. Band i., ii.
12. Schmidt's Jahrbucher. 1854, Nos. 3, 4, 5.

FRENCH.

13. Archives Générales de Médecine. 1854, Mars, Avril, Mai.
14. Revue Méd. Chir. de Paris. 1854, Mars, Avril.
15. Bull Gén. de Thérapeutique. 1854, Nos. 4—9.
16. L'Union Médicale. Mars, Avril, Mai.

ITALIAN.

17. Bulletino delle Scienze Mediche. Bologna. 1853, Nov., Déc.; 1854, Gen.

NORWEGIAN.

18. Norsk Magazin for Lægevidenskaben. Bind vii. Hefte 12; Bind viii. Hefte 1—4.

AMERICAN.

19. The American Journal of Medical Science. 1854, April.
20. The Philadelphia Medical Examiner. 1854, February, March, April.
21. The American Journal of Insanity. 1854, January.

ale, from 0·90 to 1·65; porter, from 1·80 to 2·10; stout, from 1·35 to 2·25; and cider, from 1·85 to 3·90.

2. The *sugar* was determined by Soleil's saccharometer. In sherries it varied from 4 to 18 grains in the ounce; Madeira, from 6 to 20 grains; Champagne, from 6 to 28 grains; port, from 16 to 34 grains; malmsey, from 56 to 66 grains; Tokay, 74 grains; Samos, 88 grains; Paxarette, 94 grains.

3. The *alcohol* was determined by the alcoholometer of M. Geisler of Bonn. The strength of the different samples of port varied from 20·7 per cent. to 23·2 per cent. by measure; sherry, from 15·4 to 24·7; Madeira, from 19·0 to 19·7; Marsala, from 19·9 to 21·1; claret, from 9·1 to 11·1; Burgundy, from 10·1 to 13·2; Rhine wine, from 9·5 to 13·0; Moselle, from 8·7 to 9·4; Champagne, from 14·1 to 14·8; brandy, from 50·4 to 53·8; rum, from 72·0 to 77·1; Geneva, 49·4; whisky, 59·3; cider, from 5·4 to 7·5; bitter ale, from 6·6 to 12·3; porter, from 6·5 to 7·0; stout, from 6·5 to 7·9.

II. *Upon a new Combination of Iodine.—Formulæ for a Syrup and for Solutions of Iodo-tannin.* (Bull. Gén. de Thér., April, 1854, p. 309.)

MM. Socquet and Guillermond having studied the combination of iodine and tannin, and its therapeutical effects, propose the following formulæ as those which they have found best adapted for use.

For internal use they propose a *syrup of iodo-tannin*, prepared as follows:—Iodine, 2 grammes; extract of rhatany, 8 grammes; water and sugar, of each enough to make a syrup, 1 kilogramme. The extract of rhatany employed should be prepared in vacuo, so as to be completely soluble. The iodine is first to be dissolved in a very small quantity of alcohol, and then mixed with the extract of rhatany, dissolved in the water; the whole is introduced into a glass matrass, and left for some hours; a brown powder deposits, which is to be separated by filtration, and washed with water; the strained liquors to be mixed and concentrated by a vapour bath; and lastly, the sugar is be added. Thus prepared, the syrup contains 6 centigrammes of iodine in every 30 grammes of vehicle, and may be preserved almost indefinitely without change.

For external use they propose—1. *A normal solution of iodo-tannin*, which is made by triturating together 5 grammes of iodine, 45 grammes of tannin, and 1000 grammes of water, filtering and concentrating the solution to 100 grammes. This preparation answers well as an injection into mucous canals, such as the vagina and urethra. 2. *An iodurretted solution of iodo-tannin*, prepared by triturating together 10 grammes of tannin, 5 grammes of iodine, and 90 grammes of water, favouring the solution by a gentle heat. This solution contains an excess of iodine, and is proposed to replace the several solutions of iodine made with alcohol or potash. They consider it adapted for application to ulcers of the neck of the uterus, &c.; to surfaces denuded by blistering, with a view to the absorption of the iodine; to the knee in hydrarthrosis; and, largely diluted with water, they propose that it be used for injecting serous sacs, as in hydrocele, &c.

III. *On the Operation of Glauber's Salt.* By Professor Buckheim. (Archiv für Phys. Heilkunde, von Vierordt, 1854, Erster Heft, p. 93.)

This paper is deserving of an extended notice. It furnishes the results of a series of careful experiments made to test the truth of the conjecture, that the purgative salts introduced into the intestinal canal are but slowly absorbed in accordance with their small diffusion-power, and that, arriving with a considerable quantity of water at the lower bowel, which itself usually contains consistent fæces, the peristaltic movements are accelerated by the quantity of foreign matter, and the contents of the bowels are consequently quickly expelled.* This view of

* On this subject, *vide* a paper by Aubert, noticed in No. 21, p. 297.

their operation does away with the necessity of assuming that they accelerate the peristaltic movements by a peculiar operation on the intestinal nerves. Glauber's salt and common salt were selected for experiment, the subjects being two young men, designated under the initials B. and W.

Preliminary experiments were made to determine the normal amount of excretion of sulphuric acid and chlorine in each person, by the urine and fæces.

	grammes.*	
B., on a mean, passed in his urine daily	1·741	sulphuric acid.
W., (who ate more animal food)	2·105	„
B., on a mean, passed in his urine daily	6·837	chlorine = 11·300 gr. common salt.
W. „ „	6·842	„ = 11·309 „

W.'s fresh fæces contained about 0·229 grammes of sulphuric acid, and only 0·0016 grammes of chlorine = 0·0025 grammes common salt.

The relative quantities of sulphuric acid and chlorine excreted at different periods were determined by examining the urine every three hours during the day. The sulphuric acid, tolerably uniform in quantity in the morning, increased after dinner, and was most abundant about six hours after that meal. The chlorine, small in quantity in the morning, subsequently increased, but fell immediately after food: it was most abundant three to six hours after food; but at a later period it again fell, and in the night was only one-third of the usual quantity.

Glauber's Salt.—B. and W. each took, in the morning, an ounce of this salt in three or four ounces of water. In less than an hour, active borborygmi set in; in about two hours, a watery evacuation occurred; and in the course of the day, two more of a similar character. The next day, the stools were softer than usual, and had an odour of sulphuretted hydrogen. In B, the excess of sulphuric acid in the urine, over the average, was 1·042 grammes, = 4·198 grammes of Glauber's salt. In W., on the first day, it was 0·111 grammes, = 0·447 grammes of Glauber's salt; and on the second day, 0·223 grammes, = 0·898 grammes of Glauber's salt.

In a second experiment, 15 grammes were taken, which, in about three hours, produced a watery stool. The excess of sulphuric acid

In B.'s urine on	1st	day was	1·540 grms.	= 6·205 grms.	Glauber's salt.
„ „	2nd	„	0·518 „	= 2·087	„
In W.'s „	1st	„	0·796 „	= 3·207	„
„ „	2nd	„	none.		

When 20 grammes were taken, the excess of sulphuric acid was as follows:

In B.'s urine,	1st	day,	1·290 grammes	= 5·197 grammes	Glauber's salt.
„	2nd	„	0·345 „	= 1·390	„
In W.'s urine,	1st	„	1·150 „	= 4·633	„
„	2nd	„	0·120 „	= 0·458	„
„	1st	„	1·752 „	= 7·059	„
„	2nd		—	—	
„	1st	„	1·880 „	= 7·574	„
„	2nd		—	—	

When 10 grammes were taken, lively borborygmi likewise occurred very soon, and it was with some difficulty that the urgency to stool could be repressed; both lessened, however, in three hours, and disappeared in about seven hours. On the following morning, a pappy motion occurred, smelling strongly of sulphuretted hydrogen. The excess of sulphuric acid in the urine, over the average, was as follows:

* The gramme is equal to 15·444 English grains.

1st	day, 2·061 grammes,	= 8·304	grammes Glauber's salt.
2nd	„ 0·175 „	= 0·705	„
1st	„ 1·764 „	= 7·107	„
2nd	—	—	
1st	„ 1·798 „	= 7·224	„
2nd	—	—	

From the above numbers it appears that the urine, on the day the dose was taken, and on the following day, was richer in sulphuric acid, the less the evacuations produced by the Glauber's salt, and the longer the stools could be repressed.

The experiment was now made of preventing the action of 20 grammes of Glauber's salt as soon as lively borborygmi came on, by means, in one instance, of five-eighths of a grain of acetate of morphia, and in another, by means of 20 grains of tannic acid. In the two days, the urine excreted 16 to 17 grammes of Glauber's salt; whereas, if the excretion of the bowels had been permitted, only 6·5 grammes would have been passed by the urine. In the instance in which the opiate was taken, a pappy stool occurred the following morning, which contained an excess of sulphuric acid, over the average, of 0·452 gramme, = 1·821 grammes of Glauber's salt; while in a stool the next day, no excess was discoverable. In the experiment with tannic acid, a liquid evacuation on the second morning contained 0·817 gramme of excess of sulphuric acid, = 3·292 grammes of Glauber's salt; and the total quantity evacuted by urine and fæces in the two days, was 19·939 grammes.

The next series of experiments were undertaken with a view to determine whether the degree of concentration of the salt had any influence over the result. W. took 10 grammes of Glauber's salt in 1½ ounces of water, and during the succeeding twelve hours took no liquid, and only solid food. The customary effect was produced, and the excess of sulphuric acid in the urine was,

On the 1st day, 2·097 grammes, = 8·119 grammes Glauber's salt.
„ 2nd „ 0·217 „ = 0·874 „

He now took 10 grammes without any water at all, and though much thirst was present, took no liquid for twelve hours. The borborygmi and disposition to stool were less than usual, and ceased in four hours without an evacuation; and the stool next morning was not so soft as after other experiments. The excess of sulphuric acid in the urine was,

On the 1st day, 2·124 grammes, = 8·558 grammes Glauber's salt.
„ 2nd „ 0·027 „ = 0·109 „

Next he took 4·417 grains of anhydrous sulphate of soda, = 10 grammes of crystallized Glauber's salt. A little of it was lost during the act of taking it, in consequence of coughing. It produced burning pain in the throat, and severe thirst. The borborygmi occurred as usual, and the stool next morning was pappy. The excess of sulphuric acid in the urine was,

On the 1st day, 1·376 grammes, = 2·449 grammes Anhydrous salt.
„ 2nd „ 0·372 „ = 0·662 „

He now went to the opposite extreme, taking 10 grammes of crystallized salt in six ounces of water, and drinking a great deal of water during the day. The borborygmi, &c., occurred as usual, and lasted longer than before. Next morning, the fæces were not more watery than on former occasions. The excess of sulphuric acid contained in the urine was,

On the 1st day, 2·140 grammes, = 8·622 grammes Glauber's salt.
„ 2nd „ 0·270 „ = 1·088 „

The result was, therefore, unaffected by the quantity of water taken with the salt; a conclusion similar to that arrived at by Aubert, in respect to the operation of sulphate of magnesia.

In order to show more distinctly that Glauber's salt has no purgative operation after it has left the intestinal canal and entered the blood, 15 grammes, in two ounces of water, were injected into the jugular veins of two dogs, but no effect was produced, with the exception of the fæces being rather drier than before; whereas, when given by the mouth, this dose in both animals acted freely as a purgative. Even when 20 grammes were injected, only some fever was produced. In both dogs, the sulphate was expelled by the urine, no increased quantity of sulphuric acid being discoverable in the fæces. These experiments show that the opinion, that Glauber's salt taken up into the blood, operates on the intestinal nerves, producing accelerated peristaltic movements, is incorrect.

In order to show the influence which difference of diffusion-power exerts on the rate of absorption, and thus to afford positive proof of the opinion of the author, 10 grammes of Glauber's salt were taken with 5 grammes of common salt, and the same symptoms followed as when the former was taken alone; but it was found that these salts did not appear at the same time in the urine. The common salt, which has a greater diffusion-power than Glauber's salt, was met with in the urine in proportionally large quantity in the first three hours. In the second three hours, in which the excretion of common salt lessened considerably, the quantity of Glauber's salt in the urine (which was trifling during the first three hours) increased, and reached its maximum about nine hours after injection, when the elimination of common salt had nearly ceased. The fæces passed on the day following one of the experiments, contained an excess of sulphuric acid equivalent to a quantity of Glauber's salt, sufficient, with the excess in the urine, to make up 9·817 grammes out of the 10 grammes taken; but there was no excess of common salt.

Common Salt.—Fifteen grammes of salt were taken in three ounces of water; considerable thirst was produced, borborygmi, and disposition to stool, all of which lessened in three hours; and the stools which followed at the customary time, had the usual consistence. In twenty-four hours, 9·782 grammes of salt, in excess, had passed by the urine, and a similar result followed a second experiment: the fæces next morning contained 0·0116 gramme of salt in excess. The conclusion is, that the greater part of the salt, after a short time, passes into the blood, and a part of it, in about six hours, is thrown out by the kidneys. There is this difference also between Glauber's salt and common salt, that whereas the whole of the former is discoverable in the urine and fæces, this is not the case with the latter, a fact probably due to the numerous purposes to which it may be applied in the body. The disposition of common salt to pass quickly into the blood is in accordance with its great power of diffusion; and it is those salts whose diffusion-power is least which prove useful as purgatives.

On account of the purgative salts remaining in the intestinal canal, their aperient operation can be attained by means of small doses, repeated at intervals too short to allow of their absorption into the blood. In order to ascertain the behaviour of Glauber's salt in this respect, four doses of 5 grammes each were taken every three hours, and almost all drink was avoided. Three hours after the last dose a liquid evacuation occurred, and again on the following morning. The urine contained an excess of sulphuric acid,

On the 1st day, 3·038 grammes, = 12·240 grammes Glauber's salt.
„ 2nd „ 0·230 „ = 0·927 „

The excess of sulphuric acid in the fæces was,

On the 1st day, 0·022 gramme, = 0·089 gramme Glauber's salt.
„ 2nd „ 0·886 „ = 3·570 „

In a second experiment, three doses of 5 grammes were taken with a quantity

of water. An hour and a half after the third dose, an abundant fluid evacuation occurred, and on the following morning another of pappy consistence. The excess of sulphuric acid in the urine of the twenty-four hours was,

On the 1st day, 1·505 gramme, = 6·064 grammes Glauber's salt.
„ 2nd „ 1·034 „ = 4·166 „

The fæces contained sulphuric acid in excess,

On the 1st day, 0·969 gramme, = 3·904 grammes Glauber's salt.
„ 2nd „ 0·225 „ = 0·906 „

In the first of the above experiments, during the first nine hours, not much more than 4 grammes of Glauber's salt passed into the urine, whilst in this time, 15 grammes were taken, so that about 10 grammes were contained in the canal—a quantity which experiment had shown to be insufficient for a purgative operation. The first stool occurred after the fourth dose, when the quantity of salt in the canal exceeded 10 grammes.

Assuming, now, that it is established, that the acceleration of the peristaltic movements, and the small diffusion-power of Glauber's salt are two principal factors in the operation of the latter, the question arises as to what part the mucous membrane of the intestine plays in the proceeding. Dr. Buckheim explains the watery character of the fæces, by the salt holding back a quantity of water, with which it is combined in the intestines; and he thinks that the cessation of purging with the evacuation of the salt, and the entrance of common salt into the blood as usual, are opposed to the idea of any great affection of the mucous membrane, without which, increased secretion is not to be imagined. With a view, however, to establish his opinions by experiment, and assuming that phosphate of soda or Seignette's salt acted in a similar manner to Glauber's salt, he injected some Glauber's salt into the veins of a dog, and administered one of the former salts by the mouth; no excess of sulphuric acid was found in the fæces.

The operation of Glauber's salt being now established, Dr. Buckheim made use of this knowledge to aid in determining the operation of remedies used to check diarrhœa. The experimenters took 20 grammes of Glauber's salt, and managed to repress the evacuations by repeated doses of opium and morphia; but on the second or third day colicky pains occurred, and a mucous diarrhœa with much tenesmus; all this lasting several days, in spite of opium and other remedies. The experiments undertaken showed that opium and acetate of morphia were equally effective in restraining the stools. The opium did not impede the absorption of the Glauber's salt, a fact which corresponds with results previously obtained in respect of common salt. Five grains of powdered nux vomica did not affect the operation of Glauber's salt.

Lastly, four doses of 5 grains of tannic acid were taken after a dose of 20 grammes of Glauber's salt. The borborygmi and disposition to stool were less than customary, and the evacuations were delayed. The conclusion arrived at was, that the alteration which the mucous membrane suffers from the tannic acid is accompanied by a diminution of the peristaltic movements, but less considerable than when morphia is taken.

IV. *Notice of the Leech-morass at Clairefontaine.* By M. G. Soubeiran. (Jour. de Pharmacie et de Chimie, Jan. 1854, p. 1.)

M. Borne having studied, on a small scale, the habits of this animal, has succeeded, at Clairefontaine, in rearing leeches for the market. The paper, whose title is given above, consists in an account of the arrangements of M. Borne for this purpose, and of the method he adopts of preserving, feeding, and propagating the leeches. The leech-morass is formed in a valley, with a peaty soil, in which a number of small ponds, furnished with aquatic plants, have been made, each six

metres* long, three metres wide, and one metre in depth, which he finds more convenient than ponds of a larger size.

Feeding.—M. Borne feeds the leeches with the fresh blood of oxen, sheep, or calves from the butchers, which is immediately beaten for the removal of the fibrine, and kept warm during its transit to the morass. All leeches are well fed before being introduced into the ponds, and those in the ponds are fed three times in a year—namely, in the spring, in the middle of summer, and in the autumn, just before they bury themselves in the soil to pass the winter. By adopting this last precaution, as soon as the warm weather returns, they come out and copulate, and the cocoons have the fine time of the year for hatching; whereas, if not fed till the spring, they bury themselves in the soil during digestion of the meal, copulate late in the season, and the cocoons run the risk of perishing. To feed them they are removed from the pond, put into little flannel bags, and immersed in the beat blood, and left there for an interval of from six minutes to a quarter of an hour, or half an hour in the case of the very youngest leeches. They are then taken out, washed with tepid water, then placed in fresh water, and transported again to the ponds.

Propagation.—It has been noticed that leeches make their cocoons in the soft and moist earth, at a little distance above the level of the water, and the safety of the cocoons depends on the earth remaining moist; they are destroyed, however, if the earth becomes dry, or if the level of the water rises, so as to immerse them before the exit of the young leeches. In M. Borne's ponds, therefore, the water is always kept at a constant level. It is also known that the leech hollows out in the soft soil little conduits, in which the cocoons are deposited. Accordingly, M. Borne prepares little subterranean galleries in the edges of the ponds, into which the leeches may go, and where they deposit their cocoons. In order to avoid the mingling of the young leeches with the adult ones, which would throw obstacles in the way of future management, he removes the cocoons to the border of a new pond, where arrangements are made for their shelter, and passage into the water as soon as they are born. For the details of this arrangement, of the feeding process, and of the precautions necessary to preserve the ponds from the depredations of the enemies of the leeches, we must refer the reader to the original memoir.

V. *Opium and its Adulterations.* (Lancet, Feb. 11, 1854.)

On analysis of 23 samples of gum opium as imported, 19 were found to be adulterated, 4 only being genuine; the prevailing adulteration consisting of poppy-capsule and wheat-flour; many of the samples being adulterated to a very large extent; but in 2 cases, sand, in 1, sugar, and in another, gum were discovered.

On analysis of 32 samples of powdered opium, obtained from various wholesale and retail druggists in the metropolis, only one was found to be genuine, the principal adulteration, as in the previous case, being with poppy-capsule and wheat-flour. Four of the samples were further adulterated by the addition of powdered wood, introduced, no doubt, in the process of grinding.

According to the analysis of gum opium, as imported, the amount of alkaloids was found to vary from 2·7 to 14·0 per cent.; that is, in the proportion nearly of one to five. It is probable, however, that the specimen of Egyptian opium, which furnished only 2·7 per cent. of alkaloids, had been deprived of its morphia; and it was also adulterated with an enormous quantity of some gummy substance. The two gum opiums which furnished the next lowest amounts, were another sample of Egyptian opium, which contained only 3·7 per cent., and a sample of Turkey opium, which yielded but 4·2 per cent. of alkaloids.

The amount of alkaloids in the samples of powdered opium varied from 2·3 up to 12·2 per cent., or in the proportion of nearly one to six. The lowest amounts of alkaloids furnished by the powdered opium were 2·3 and 3·2 per cent.; these were, in all probability, exhausted opiums, which had been previously employed in the preparation of tincture.

* The metre = 1·0936 English yards.

VI. *The Application of Histology, or the Science of Tissues, to Pharmacy.* By Mr. HENRY SUGDEN EVANS. (Pharm. Jour., vol. xiii. pp. 309 and 408.)

WE only regret that it is out of our power to do more than refer our readers to two valuable papers which have appeared in recent numbers of the 'Pharmaceutical Journal,' on a subject at once novel and interesting. The result of such inquiries as these cannot fail to be of importance both to the physician and to the druggist, in the detection of those sophistications of which powdered drugs especially are known to be the subject. The drugs whose microscopical characters are described, are rhubarb, ipecacuanha, white hellebore, turmeric, ginger, and jalap.

VII. *Essay on the Preparations of Squill.* By Dr. CHATEAU. (Archives Générales, Jan. 1854, p. 53.)

THE object of the experiments of Dr. CHATEAU was a reply to a question proposed by the Faculty of Medicine of Paris—"*Determine by clinical observations what are the effects of the preparations of squill.*" In a résumé of the memoir presented to the Academy, M. Chateau states the powder, wine, vinegar, and oxymel of squills were employed; but the powder most frequently, on account of its action not being interfered with by any other constituent of the medicine.

His first *experiments were made upon seven dogs*, to whom he administered quantities of the squill varying from 40 grammes of the powder to 1 gramme, or their equivalent, of the wine of squill. In one of these experiments, 1 gramme of soft extract of squill was injected into the subcutaneous cellular tissue; in all the other experiments, the drug was introduced into the stomach. The effects produced were the following: The animals became dull, and this was followed by increase of the buccal secretions, nausea, efforts at vomiting, and by semi-fluid stools passed in small quantities; when the dose was sufficiently large, tremblings supervened, and paralysis of the posterior limbs, which soon extended to the anterior. After this, the animals appeared to regain their equilibrium, and then suddenly a convulsive attack occurred; they fell upon their side; there were some movements of deglutition, a little orthotonos, and death in from thirty-five minutes to one hour and fifty-five minutes after administration of the drug. When the dose was small, the same series of phenomena was exhibited, but more tardily; and death was delayed for twelve or fifteen hours. In all the experiments there was a remarkable diminution of the temperature of the body, as taken in the rectum, believed to be due to the hyposthenic action of the squill upon the nervous system. On *examination of the bodies after death*, the viscera were found congested, the blood black and thick, the bladder empty, or containing but little urine, the ganglia of the great sympathetic reddened, the cerebrum little altered, but the cerebellum and spinal cord softened sometimes even to diffluence.

M. Chateau next relates the results of his *observations on the human subject in disease*, which had reference principally to the diuretic and laxative operation of the drug. He administered it in 4 cases of pulmonary emphysema, in 3 cases of albuminuria, 3 cases of abdominal dropsy, 1 of which was ovarian, 2 cases of pleurisy, 2 cases of pneumonia, 2 cases of phthisis, and several cases of rheumatism, only 1 of which is related in the original memoir. In these 17 cases, the operation of the squill was as follows:—In 7, it proved *diuretic and purgative*; in 2, simply *diuretic*; in 2, simply *purgative*; in 4, *expectorant*; in 1, *diuretic and expectorant*; and in 1 no effect was observed.

He noticed that when either the purgative or diuretic operation became excessive, the other immediately ceased.

The number of cases of each disease in which the squill was administered was too small to warrant the general therapeutical conclusions drawn by Dr. Chateau; we shall therefore content ourselves with noticing two points of interest con-

nected with the operation in the cases described. Of the cases of *albuminuria*, 2 were purged by the powdered squill without any increase in the secretion of urine; in the third case, where the wine of squill was employed, a diuretic effect followed; but in all three cases, the albumen in the urine was unaffected. In one of the cases of *abdominal dropsy*, where the powder proved diuretic and laxative, the dropsy was removed in less than two months.

The reply of Dr. Chateau to the question of the Academy is, "*That squill has a direct hyposthenic action upon the ganglionic and cerebro-spinal systems, and that this action is exhibited at first by an increase of the urinary and intestinal secretions, and at last, if the dose of the drug be large, by paralysis and death.*"

Dr. Chateau prefers the powder for internal administration, next to this the wine of squill, and as an expectorant, the oxymel. He considers 10 to 15 centigrammes (from $1\frac{1}{2}$ to $2\frac{1}{4}$ grains) a good *dose* to commence with, and that it may be increased, after some days, to 35 or 40 centigrammes ($5\frac{1}{2}$ to 6 grains).

VIII. *Influence of Digitalin upon the Excretion of Urea.* By G. SIEGMUND. (Archiv für Pathol. Anat. und Phys., und für Klinische Medicin, Bd. vi. Heft 2, p. 238.)

THE experiments related in this paper were made upon rabbits. The author, in each instance, first ascertained the quantity of urine and of urea normally secreted by the animal, and subsequently the quantity under the influence of the digitalin. The results are given in tables, which represent the actual quantity of food taken, and of excretion voided, and also the proportion of the latter to the former.

Rabbit 1.—The following table represents the results of four days' observation prior to the use of the digitalin.

	Total of 4 days.			Mean for 1 day.	
Food	800	grammes.	...	200	grammes.
Quantity of urine	569	cub. centim.*	...	142·3	cub. centim.
Weight of urine .	582·6	grammes ...	...	145·6	grammes.
Urea	10·448	„ ...	...	2·612	„
Fæces	0·6	„ ...	...	0·15	„

1 gramme food	= 0·71	cub. centim.	urine.
„ „	= 0·73	gramme	„
„ „	= 0·013	„	urea.
100 cub. centim. urine	= 1·84	„	„

The dose of digitalin administered was at first one-sixteenth of a grain, but it was soon increased to 2 grains per diem. It produced no signs of depression; but, on the contrary, violent temporary disturbance, manifested particularly by acidity of the urine. The only striking effect upon the pulse was irregularity. The use of the digitalin was prolonged over ten days, and the following table exhibits the results:

	Total of 10 days.			Mean for 1 day.	
Food	1964	grammes.	...	196·4	grammes.
Quantity of urine	1506	cub. centim.	...	150·6	cub. centim.
Weight of urine .	1541·8	grammes.	...	154·18	grammes.
Urea	21·434	„ ...	...	2·143	„
Fæces	3·7	„ ...	...	0·37	„

1 gramme food	= 0·76	cub. centim.	urine.
„ „	= 0·78	gramme	„
„ „	= 0·011	„	urea.
100 cub. centim. urine	= 1·42	„	„

From which it appears, that, with nearly equal quantities of food, the quantity of

* A cubic centimetre = 0·061 cubic inch, or 0·001761 pint.

urine passed under the influence of the digitalin was increased by 8·3 grammes; but that, notwithstanding, 0·5 gramme less urea was excreted daily than before its administration.

Rabbit 2.—Observations prior to use of digitalin.

	Total of 4¾ days.			Mean for 1 day.	
Food	1210	grammes.	...	254·7	grammes.
Quantity of urine	912	cub. centim.	...	192·0	cub. centim.
Weight of urine .	924·9	grammes.	...	194·7	grammes.
Urea	13·225	,, ...	...	2·784	,,
Fæces	0·6	,, ...	...	0·126	,,

1 gramme food = 0·75 cub. centim. urine.
,, ,, = 0·76 gramme ,,
,, ,, = 0·0108 ,, urea.
,, ,, = 0·0005 ,, fæces.
100 cub. centim. urine = 1·45 ,, urea.

Under the use of similar doses of digitalin to those given to the first rabbit, still less effect was produced upon the pulse. There was no general disturbance, and the urine was never acid. In neither of the animals was the frequency of micturition affected. The following are the results of four days' administration of the digitalin:

	Total of 4 days.			Mean for 1 day.	
Food	1311	grammes.	...	327·8	grammes.
Quantity of urine	1015	cub. centim.	...	253·3	cub. centim.
Weight of urine .	1031·3	grammes.	...	257·8	grammes.
Urea	14·095	,, ...	...	5·524	,,
Fæces	1·3	,, ...	...	0·33	,,

1 gramme food = 0·78 cub. centim. urine.
,, ,, = 0·78 gramme ,,
,, ,, = 0·0107 ,, urea.
,, ,, = 0·0009 ,, fæces.
188 cub. centim. urine = 1·33 ,, urea.

For each gramme of food then taken, there was an increase of 0·03 cub. centim. of urine excreted. In this instance, however, the quantity of urea excreted, in relation to the food, was not remarkably lessened (as observed in the former case), although its proportion, in a given bulk of urine, was lessened, in consequence of the augmented excretion of water.

IX. *On Delphinin.* By J. LEONIDES VAN PRAAG. (Archiv für Pathol. Anat. und Physiol. und für Klinische Medicin, Bd. vi. Heft 3, p. 385.)

THE deficiency of our knowledge respecting the properties and operation of the alkaloid delphinin, has led the author of this paper to their new and complete investigation. The delphinin which was employed in the experiments, whose results we are about to detail, was obtained from Messrs. Frommsdorf, of Erfurt.

Dr. J. L. VAN PRAAG administered the alkaloid to animals from each class of the vertebrata, and in the paper under our notice, gives the results of his observations on fishes, frogs, birds, and mammalia, and relates at length the experiments which he instituted. The mammals which he selected for experiment were dogs, cats, and rabbits. In these, asphyxia, as a result of paralysis of the heart (*Herzlähmung*) was among the principal phenomena; whilst paralysis (*Lähmung*) of the nerves of motion, and, at a later period, of those of secretion and the special senses followed. Dogs were least of all affected when the poison was given to them in a piece of meat, since the two were soon vomited together. It operated

more powerfully when it was administered by the anus, or introduced into a wound of the integument. But death followed almost suddenly, so that scarcely a minute intervened, upon injection into a vein, from arrest of the heart's action; the animals, in consequence of discontinuance of respiration, gasp for breath with open mouth, and in a short time death follows, in the midst of severe tetanus. On injection of the alcoholic solution into the anus in dogs, repeated evacuation of fæces at first occurred, and then salivation. The walk became staggering and uncertain. There arose great adynamia, in consequence of which they leaned against anything for support, until at last they fell down. The sensibility of the skin diminishes simultaneously with the lessening power of the motor nerves, until anæsthesia is complete. The respiration is at first increased, panting and accompanied by howling; in addition to this, tenacious, clear mucus accumulates in the glottis and larynx, which renders respiration still more difficult. At a later period, the frequency of respiration diminishes, until at last the act occurs very rarely, or ceases altogether. The functions of the brain and of the organs of the senses are little affected, until the disturbance of the circulation and respiration increases. In the instance of cats, into whose anus the alcoholic solution was injected, the evacuation of fæces and flow of saliva were also increased, and there were observed, at first, frantic and wild leaps, the symptoms of adynamia occurring at a later period. The phenomena of the circulation and respiration are the same as in dogs, and both cats and dogs show signs of a tickling sensation in the mouth. In rabbits, also, after the injection into the anus, active and strong symptoms of reaction appear, as in cats, terminating, however, in adynamic symptoms. When the delphinin, in a fatal dose, was introduced, in cats, into the subcutaneous cellular tissue of the back, the poison operated first of all on the sensory nerves at this spot, and induced symptoms resembling those arising from a powerful irritant. The animals exhibited great disquiet, bristled up their hair, set up their back, rolled on their back, and finally lay down. Gradually, however, in this case also, the action of the poison upon the heart and lungs appeared. When, in the instance of dogs, the poison was given dry with the food, the operation of the poison soon became apparent upon the mucous membrane; there were observed vomiting, and an excessive itching in the mouth, so that the animals rubbed their lips and mouth with their feet, and rubbed their nose upon the ground. With this, also, arose profuse salivation. The principal points observed on dissection of the poisoned animals were, fulness with blood of the membranes of the brain, of the heart, of the larger venous trunks, and of the liver; fulness of the gall-bladder, and a collection of mucus in the air-passages. Nothing was noticed respecting the condition of the kidneys. In one case, air was found in the veins of the membranes of the brain.

X. *Some Remarks on the Vegetable Astringents, with special relation to the Root of the Arbutus Unedo.* (Bull. Général de Thérap., April, 1854, p. 304.)

M. Guyot Dannecy having presented a memoir to the Société de Pharmacie, in which he drew the conclusion, that various preparations of the root of the *arbutus unedo* might in all cases be substituted for extract of rhatany, M. Soubeiran was deputed to report upon this subject, and was led to make observations upon the vegetable astringents most frequently used, with a view to determine the place which the extract of arbutus should occupy amongst them. 1. The degree of astringent impression made by solutions of a certain quantity of the different extracts upon the mouth, gave rise to their arrangement in the following order of sapidity:—Pegu catechu; Jamaica catechu; Amboyna kino; Indian catechu; extract of rhatany; extract of monesia; extract of tormentil; extract of oak-bark; extract of bistort; extract of arbutus-root. 2. The colour produced by perchloride of iron, with the different solutions, was as follows:—Pegu catechu, green; Indian catechu, extract of monesia and the two kinds of kino, brown;

extract of rhatany, dirty grey; the others blue. In order to obtain an approximative estimate of the proportion of tannin in each, he diluted each of these coloured liquids with water, until the colour ceased to be evident, and found that one million parts of water ceased to be coloured when the solution contained only 8 parts of Pegu catechu, 10 of Jamaica kino, 12 of Amboyna kino, 14 of Indian catechu, 15 of extract of monesia, 15 of extract of rhatany, 35 of extract of tormentil, 50 of extract of bistort, 55 of extract of oak-bark, and 160 of extract of arbutus-root. The order of astringency thus attained corresponds with the effects of each as observed in medical practice.

XI. *On Poisoning by Solanin.* By Dr. FRAAS. (Archiv für Pathol., Anatom., und Physiol., und für Klinische Medicin, Bd. vi. Heft 2, p. 226.)

AN opinion has been prevalent, that certain diseased states in domestic animals arose from the deleterious action of solanin contained in potatoes, on which they had been fed. In testing the truth of this opinion, Dr. FRAAS made the experiments, of which the following account is an abstract. Otto had previously found that the ripe potato, unsprouted, contained only a trace of solanin, that more of this principle was discoverable in the stalk of the plant, but the greatest quantity in the sprouts, especially the shorter ones. A similar result was obtained by Berchtold, in the spring of 1853. In 100 grammes of the green potato-sprouts, he discovered 4 milligrammes of solanin; and another time, in the very short sprouts, 12 milligrammes.

The first observations on animals were made on two pigs, to one of which were given sprouted potatoes boiled, together with the water in which they were boiled, which necessarily contained all the soluble solanin; while to the other were given the boiled potatoes without the liquor, the food being, consequently, assumed to be free from solanin. This was continued from the 15th of April to the 8th of July, without any signs of ill health appearing in either animal, notwithstanding that the outbreak of the disease in pigs was mentioned in the journals as having appeared.

The next observations were made with pure solanin, 10 grains of which were given to a pig, without the least injurious result, and the next day 20 grains were given to the same animal, with no further result than producing a diminished appetite and frequent white thin stools, the animal being quite well again the next day. On the third day, 20 grammes of acetate of solanin were given without effect.

Five grains of pure solanin were given to two dogs, the only results of which were vomiting and dilatation of the pupil. Ten grains were now given, and the œsophagus was tied; strong efforts at vomiting were made, and there was much vascular excitement, but the dose was not fatal.

The sulphate of solanin was injected into the veins of dogs. In nine dogs, death resulted from doses of from 5 to 2 grains thus administered; smaller doses only served to excite the circulation. In the case of one dog, into whose right jugular vein 5 grains were injected, respiration suddenly became difficult, accelerated, and spasmodic, with convulsions and tetanic extension and drawing back of the head, and death occurred in seven minutes.

Two grains of solanin were injected into the right jugular vein of a horse without any result being observed. Into the jugular vein of another horse, 30 grains of sulphate of solanin were injected. The animal was suddenly attacked with severe difficulty of breathing, and exhibited a great increase in the action of the heart, with convulsions, so that he appeared to be dying. In the course of twenty minutes he appeared quite recovered.

Two grains of acetate of solanin were injected into the rectum of a rabbit. The symptoms produced were heaviness, apathy, and slowness of movement; dilatation of the pupil followed, increased activity of the circulation and respiration, and

convulsions, which, however, ceased in about two hours' time. The animal now moved but little, but when it did attempt locomotion, it dragged itself along with difficulty; but no special loss of power of the hinder extremities was observed. It died in six hours.

To a second rabbit, a certain quantity of acetate of solanin was given daily by the mouth, commencing with one grain. After several days, no effect being produced, the dose was increased to 2, and after another interval, to 3 grains. Some days after this, the appetite lessened, but no other alteration being observed, the dose was increased to 4 grains. At last, the animal became heavy, slow in its movements, remained lying the greater part of the day, and at last died, without paralysis or any considerable fever having been observed.

Dr. Frans is inclined to the opinion, that the pernicious effects which often follow feeding upon raw potatoes and potato-stalks, are due to the great quantity of alkaline and earthy salts which they contain, and never to the solanin. An analysis of the potatoes used in his experiments, gave the following results: In 100 parts of dried potatoes there were 4·22 per cent. of ash; or in 100 parts of fresh potatoes, 1·17 parts of ash. 100 parts of ash contained—

Sulphuric acid	2·90
Phosphoric acid	12·37
Silicic acid	A trace.
Chlorine	4·23
Potash	52·23
Soda	A trace.
Alumina	A trace.
Oxide of soda	A trace.
Magnesia	2·41
Lime	3·68
Carbonic acid	20·18
Loss	2·0
	100·00

The potatoes contained no solanin.

ANNALS OF PHYSIOLOGY.

By Hermann Weber, M.D.,
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I. Food and Digestion.

Researches on the Organs of Digestion and Absorption. By Dr. Donders. (Zeitschrift für rat. Med., iv. 2, p. 30. 1853.)

Donders has performed some experiments in order to elucidate the influence of the *pancreatic juice* on the digestion and assimilation of fat. The result of these experiments is in accordance with those of Frerichs, Bidder, Schmidt, Lenz, and Herbert,* proving the incorrectness of Bernard's† view, that the fatty matters of the food are emulsified, and thus made fit for absorption by the action of the pancreatic juice.

* See British and Foreign Medico-Chirurgical Review, No. 25, pp. 257, 258.

† Comptes Rendus Hebd. de l'Académie des Sciences, tom. xxvii. p. 249.

II. Respiration and Circulation.

1. *Further Contributions to the Physiology of Respiration and Circulation.* By Dr. Donders. (Henle und Pfeuffer, Zeitschrift für rat. Medicin., iv. pp. 241. 1853.
2. *De Spirometro ejusque usu, &c. &c. Dissertatio.* By Dr. Fabius. Amstelodami. 1853.
3. *Objections to the Formula for the Vital Capacity of the Lungs according to* Buys, Ballot, *and* Fabius. By Dr. Donders. (Zeitschrift für rat. Med., iv. pp. 304. 1853.)
4. *On the Relation between the Frequency of the Pulse, the Lateral Pressure of the Blood, and the Velocity of the Circulation.* By Dr. Lenz. (Inaug. Dissert.) Dorpat, 1853.
5. *On the Functions of the Auricles of the Heart.* By Dr. Wachsmuth. (Zeitsc. für rat. Med., iv. pp. 182. 1853.)
6. *Figurative Representation of the Arterial Pulse.* By Dr. Vierordt. (Vierordt's Archiv, xiii. pp. 284. 1854.)

1. An analysis of Donders' former essay on the 'Mechanism of Respiration and Circulation," is given in the January number of this journal.* The present essay relates to some phenomena connected with an *increased* or *diminished respiratory pressure.* The effect of the *increased* respiratory pressure (compression of the thorax with prevented escape of air) was—1. In 7 out of 9 cases, increase of frequency of the pulse; in 2 out of 9, decrease. 2. In all cases, great weakness of the pulse and of the sounds of the heart. 3. In most cases, complete imperceptibility of the sounds of the heart. 4. In 2 out of 9, also, the pulse became imperceptible. Donders differs, therefore, from Ed. F. Weber,† in having found *increase* of frequency of pulse as the general effect, while Weber had found *decrease;* on the other points, both authors are in accordance. He explains the weak action of the heart by the scanty flow of blood to the right cavities. The principal phenomena produced by *diminished* (*negative*) *respiratory pressure* (deepest possible inspiration) were—1. In all cases the pulse became less frequent and weaker. 2. In some cases, disappearance of the sounds of the heart, and even of the pulse, if the lungs are kept for some seconds in the state of the deepest inspiration. We have no space to enter more fully into the explanation of these phenomena, as proposed by Donders. The chief points are, that the lessened pressure on the heart and thoracic vessels is considered to lead to an increased flow of the venous blood to the right heart, in consequence of which at first more vigorous contractions are excited, but soon, through the negative pressure, the left ventricle is prevented from expelling the blood, the heart remains in the state of diastole, &c. &c.

2, 3. The method of Buys-Ballot and Fabius is based on—1. The height of the trunk (from the protuberant occiput to the os coccygis); 2. The circumference of the thorax in the height of the nipples; 3. The mobility of the chest—*i.e.*, difference between the deepest inspiration and expiration; 4. The age of the person. Of these four points, according to the method of the smallest quadratio, a formula was constructed. The height being called l, the circumference a, the motility m, and the age v, the formula would be:

$$l \times a \times (502 + 16{\cdot}5m + 0{\cdot}37m^2 - 2{\cdot}5(35 - v)).$$

Without further entering into the correctness of this formula, Donders objects that it can show, at the utmost, what the vital capacity *is*, but not what it *normally ought to be.* By diseases, by the occupation and exercise, the various factors

* Two important errata must be noticed in the analysis of January:
1. p. 262, line 30, *read* 7·5mm, *instead of* 9·5mm.
2. p. 262, line 45, *read* forced expiration, *instead of* forced inspiration.

† Müller's Archiv, 1851, p. 88.

of the formula may be altered. As, for instance, the circumference of the thorax may become considerably increased by gymnastics; the formula would then calculate for such a case the vital capacity too high. Donders prefers, therefore, the old method of Hutchinson to that of Fabius.

4. LENZ performed his experiments on calves and dogs, making use of the *hæmadromometer* and *hæmadynamometer*. To examine into the relation between the three factors in question, he altered the frequency of the contractions of the heart by acting on the vagi, by introducing into the system, digitalin, tartrate of antimony, and chloroform. 1. *Irritation of the Vagi* produced (*a*) retardation of the pulse; (*b*), diminution of the lateral pressure of the blood; (*c*), diminution of the velocity of the circulation. 2. *Section of the Vagi:* (*a*), increase of the frequency of the pulse; (*b*), lateral pressure at first increased, then irregular, then sinking below the standard; (*c*), velocity changed in a similar manner. 3. *Digitalin:* (*a*), by small doses, decrease, by large doses, increase of frequency of the pulse; (*b*), lateral pressure at first increased, while the frequency of the pulse is decreased, then it gradually sinks. The increase of frequency was at first synchronous with increased pressure. (*c*), The velocity of the blood did not exhibit any remarkable change. 4. *Chloroform* (through the stomach to complete narcosis): (*a*) frequency of the pulse unchanged; (*b*), lateral pressure exhibited scarcely any abnormal alteration; (*c*), velocity much diminished. 5. *Tartrate of Antimony* (injected into the veins): (*a*), pulse in some cases scarcely influenced, in others very irregular and intermittent; (*c*), velocity in most cases at first decreased, then increasing. 6. In *normal cases*, great fluctuation in the frequency of the pulse, without any remarkable alterations in the two other factors. From all these observations and experiments, it results that, as yet, no relation can be traced between the frequency of the pulse, the lateral pressure, and the velocity of the blood.

5. In opposition to the theories of Baumgartner, Hamernjk, Nega, and also to that of Skoda, and the older schools, Dr. WACHSMUTH explains the actions of the valves of the heart, by assuming "that the gradually increasing resistance against the vis a tergo suddenly becomes preponderating by the remission of an active increase of the latter, which may be accompanied or followed, or may not be so, by an *active* increase of the resistance." According to Wachsmuth's view, the mechanism is the following. The auricle is filled and in the state of diastole at the close of the systole of the ventricle, as soon as the diastole of the latter takes place, a part of the blood contained in the auricle must enter into the ventricle; in consequence of this, the auricle contracts, or better, diminishes in size, becomes more narrow without muscular action (by passive contraction). In the meantime, the vis à tergo continues to convey the blood from the veins to both cavities, and only towards the close of the diastole of the ventricle an active muscular contraction of the auricle takes place. The sudden remission of the latter leads to the preponderance of the resisting power of the blood in the ventricles, effecting the sudden closure of the auriculo-ventricular valves, which process is, of course, much assisted by the ensuing contraction of the ventricles. Through this not only the complete closure is explained, but also the production of a musical note. The principal function of the auricle, therefore, is that of regulating the filling of the ventricles, forming the intermedium for the admission of the blood from the veins into the ventricles, and thus preventing the contraction of the ventricle being an impediment to the circulation; the short obstacle opposed to the flowing-off of the venous blood by the auricular contraction, produces, on account of the elasticity of the vessels, no venous pulsation, nor does it hinder the circulation.

6. Dr. VIERORDT has constructed an instrument by which the phenomena of the arterial pulse (degree of motion of the artery, time of systole and diastole, &c.) are accurately delineated, such as to show, at once, in a figure, the rhythm, the velocity, the degree of equability of the single pulsations, &c. &c. We refrain, at present, from a description of the instrument, as we shall have occasion to return to this subject when Vierordt has published the result of his observations and experiments.

III. Lymphatic System and Ductless Glands.

1. *Researches concerning the Organs of Digestion and Absorption.* By Dr. Donders. (Henle und Pfeüffer's Zeits. für rat. Med., iv. pp. 230.)
2. *On the Minute Structure and the Formation of the Lymphatic Glands.* By Professor Kölliker. (Verhandl. der Physic. Med. Gesell. zu Würzburg, iv. 2. 1854.)
3. *On the Spleen, and some Peculiarities of its Capillary System.* By Dr. Fuhrer. (Vierordt's Archiv. Band xiii., pp. 149. 1854.)

1. Donders found the *sheath* of the lymphatic glands to consist of a thin fibrous layer, in which, however, he could not detect the fibrous cells and muscular fibres, as described by O. Heyfelder, nor was he able to excite any contraction by means of galvanism. From the sheath filaments enter into the interior of the glands, dividing the whole more or less completely into lobules, which, by further splitting of the fibrous filaments, are subdivided into smaller and smaller partitions. Within these partitions, the walls of the lymphatic vessels are very thin and indistinct; they contain holes of various sizes, some of which are large enough to permit the entrance of cells from the parenchyma. Donders supposes from this, that the lymphatic glands are fabrics of the lymph-corpuscles; that particles from the parenchyma, which is in a constant state of change, and in an intimate connexion with the blood through the rich distribution of capillaries, constantly enter into the lymphatic vessels, while some constituents of the chylus and lymph become admixed to the blood. With Brücke and Frey, Donders is inclined to consider the glandulæ Peyeranæ as lymphatic glands. The researches of Dubois-Reymond, and Wiedemann make it to him very probable, that the function of secretion as well as that of absorption, through the lymphatic vessels, is in close connexion with galvanic currents.

2. Kölliker considers the lymphatic glands as consisting of *sheath*, *cortical*, and *medullary substance*. His description of the structure of the *sheath* is similar to that of Donders; the partitions of *cortical* substance are called by him *alveoli;* their size varies from $\frac{1}{8}'''$ to $\frac{1}{2}'''$. These alveoli contain a grayish pulpy substance, consisting of a very tender vascular network, of spindle-like, radiating cells, anastomosing one with another, and of an alkaline juice, in which round cells are floating, much like those of the lymph or chyle (0·003'''—0·005'''). This juice appears to him to be not a peculiar component of the glands, but chyle or lymph. The *medullary substance* consists of the larger ramifications of the bloodvessels, and a copious plexus of lymphatic vessels, connected together by a dense intercellular tissue, without any alveolar structure. Concerning the course of the lymphatic vessels, they pierce, as vasa inferentia, the sheath of the gland on several points, ramify into finer and finer branches towards the alveoli, into which they probably open; the alveoli thus forming not mere excavations, but *corpora cavernosa lymphatica.* From these excavations, lymphatic vessels with walls take again their origin, proceed into the medullary substance, forming there the vascular plexus, from which they issue as vasa efferentia. The view of the lymphatic vessels really opening into the alveoli, and of the latter being a continuation, a part of the lymphatic vessels, is principally founded on the following points:—1. That no lymphatic vessels can be discovered within the spongy tissue of the alveoli. 2. That the cells are freely floating within the contents of the alveoli. 3. That by injection at first, the vasa inferentia and the alveoli are filled, and only by increased force also the vessels of the medullary substance and the vasa efferentia. The smallest ramifications of the arteries form a very rich meshwork within the walls of the alveoli; the lymph and chylus contained therein are therefore in a constant interchange with the blood; the lymphatic glands can, on that account, not be considered as mere plexuses of lymphatic vessels, but they are organa *sui generis,* for which the term "*glands*" ought to be retained. Their *chief function* is the formation of the greater part of all the lymph- and chyle-globules, which, no

doubt, takes place principally within the alveoli, where the lymph, being under a lower pressure than the blood, constantly receives from the latter some of its ingredients. That this growing of cells must be connected with a *chemical change* in the blood and lymph cannot be doubted; but Kölliker does not venture to proffer anything in this respect beyond what is generally adopted. He alludes to the faculty of the lymphatic glands to contract and to become larger (through the contractile elements of the blood and lymphatic vessels), as being important for the performance of their function, without, however, entering into details.

3. Führer considers the spleen to be at the same time an organ of *retrogressive metamorphosis*, as also a *laboratory* of the *blood-globules*. Concerning the first function, the spleen acts like the other secreting glands, only with this exception, that the products of the metamorphosis are not excreted by a proper duct, but are carried off with the blood by the vena lienalis, and conducted to other organs of secretion, to undergo there further change. The blood-globules are formed, according to Führer's view, within a peculiar adventitious system of capillaries. These adventitious transitory vessels originate from the normal capillaries themselves, analogous to the new vessels in pseudo-membranes. They commence as small excrescences, which elongate into thin processes, forming on some points of their course various swellings, cellular cavities, in which the blood-globules are developed. New ramifications proceed from these, forming anastomoses with others, and communicating through these with veins, into which they pour out their contents. As soon as the circulation and formation of blood becomes sluggish, these adventitious vessels collapse, and a part of them perishes altogether; on the other side, their development becomes very active, connected with swelling of the whole spleen, and increase principally of the Malpighian bodies during and soon after the process of digestion—i. e., during the period of a more energetic tendency of the blood to the spleen, with increased activity in the formation of blood. The peculiar fibrous cells (Faserzellen) described by other authors as forming part of the parenchyma of the spleen, are cells or cellular bodies of the just-described adventitious vessels; they are, therefore, fragments of the apparatus for the formation of blood, the nuclei seen within them represent the commencement of new blood-globules. As corroborating his view, Führer refers to the phenomena consequent on the extirpation of the spleen—i. e., vicarious swelling of the lymphatic, principally mesenteric glands, which exhibit an unusual development of the normal capillary vessels, and besides this, the adventitious vascular system just described in the spleen.

IV. Metamorphosis of Matter, Organs of Secretion and Excretion.

1. *The Metamorphosis of Matter during the Deprivation of Water.* By Drs. Falck and Scheffer. (Vierordt's Archiv für Phys. Heilk., xiii. pp. 61 ss. 1854.)
2. *On the Doctrine of the Secreting Cell.* By Dr. Luschka. (Vierordt's Archiv, xiii. 1854.)
3. *Researches concerning the Organs of Digestion and Absorption.* By Dr. Donders. (Zeits. für rat. Med., iv. pp. 230 ss. 1853.)
4. *On the Influence of Digitaline, and Section of the Vagus on the Excretion of Urea.* By Dr. Siegmund. (Virchow's Archiv, vi. 2, pp. 238 ss. 1853.)
5. *On the Influence of the Pressure of the Blood on the Secretion of Urine.* By Dr. Goll. (Zeits. für rat. Med., iv. 1, pp. 78 ss. 1853.)
6. *On the Faculty of Absorption of the Human Skin.* By Dr. Homolle. (L'Union, Nos. 117, 118, 119, 1853; and Schmidt's Jahrb., 1854, iii. pp. 290 ss.)

1. Falck and Scheffer performed their experiments on pigeons kept as much as possible under equal circumstances. The weight of the animals, of the food consumed, and of the excreta (collected on glass plates), was daily noted. The annexed table will exhibit the points of interest in the shortest and most palpable

manner. Under A are the figures belonging to pigeons I. and II., who were supplied with solid and fluid food *ad libitum;* under B, those of the same pigeons deprived of fluid food; under C^1, C^2, C^3, those of the same pigeons, supplied with solid and fluid food after they had been deprived of the latter during four days (under C^1 the numbers of the first day of supply; under C^2 the average numbers of the first four days; under C^3 those of the four following days, *i.e.*, from the 6th to the 9th inclusive); under D, those of the same pigeons dying from want of fluid food. The weight of the pigeons is reduced to 1000, and the other figures in the same proportion.

	A.		B.		C^1.		C^2.		C^3.		D.	
	P. I.	P. II.	P. I.	P. II.	P. I.	P. II.	P. I.	P. II.	P. I.	P. II.	P. I.	P. II.
Water . .	97	109	0	0	317	267	149	148	87	95	0	0
Wheat . .	83	88	19	32	80	80	104	96	88	82	19	19
Ingesta . .	180	197	19	32	397	347	253	244	175	177	19	19
Excreta . .	66	73	11	19	56	74	69	80	72	57	12	14
Perspiration	114	121	69	60	135	96	130	123	103	120	42	44
Egesta . .	180	194	80	79	191	170	199	203	175	177	54	58

It scarcely requires to be remarked that the word perspiration is used by the authors for the loss through the lungs and the skin.

We farther extract the following figures:—

	A.	B.	C^1.	C^2.	C^3.	D.
In Pigeon I.:						
Ingesta : Egesta	100 : 100	100 : 418	100 : 50	100 : 78	100 : 100	100 : 282
Ingesta : Excreta	100 : 37	100 : 59	100 : 15	100 : 27	100 : 41	100 : 61
Ingesta : Perspiration . . .	100 : 63	100 : 359	100 : 34	100 : 51	100 : 59	100 : 220
In Pigeon II.:						
Ingesta : Egesta	100 : 99	100 : 245	100 : 49	100 : 83	100 : 100	100 : 300
Ingesta : Excreta	100 : 37	100 : 59	100 : 21	100 : 33	100 : 32	100 : 73
Ingesta : Perspiration . . .	100 : 62	100 : 186	100 : 28	100 : 50	100 : 68	100 : 227

The figures under C^1 show how much the ingestion of water is increased on the first day after privation of fluid food, while the egestion does not surpass the standard in the same proportion. C^2 shows that also during the following days the water is still consumed in a larger quantity, the difference being, however, not so great; also the ingestion of solid food exceeds the average; the sum of the ingesta overweighs, therefore, that of the egesta. In C^3 we observe both returning to the normal proportion. B and D make it evident that during the privation of water also the quantity of solid food consumed is much below the average. Pigeon I. died in 12 days, pigeon II. 13 days after the commencement of the privation of water; loss of weight in I. 44·5 per cent., in II. 47·2. The post-mortem examination exhibits the phenomena of death from asphyxia.

2. While it is generally admitted that the secretion of bile, semen, with gastric juice and that of several other glands, is effected by means of a metamorphosis of *cells*, the products of the plexuses of the brain, the pleura, the pericardium, peritoneum, and membrane hyaloidea, are by most physiologists considered as having their origin in *simple transudation of the plasma sanguinis* through the coats of the vessels. LUSCHKA doubts the correctness of the latter view, ascribing the secretion also of the serous fluids (cerebro-spinal fluid, humor pleuræ, pericardii, peritonei, and the humor aquosus and vitreus) to the agency of *secreting cells*. In favour of his view he draws attention to the great physical and chemical difference between these fluids and the liquor sanguinis; to the limited quantity of fluid con-

tained in those cavities during the state of health, while no mechanical impediment for farther transudation can be recognised. Besides this, Luschka succeeded in detecting cells in connexion with all the structures in question, the physical and chemical constitution of which must tend to corroborate his view. The plexuses of the ventricles of the brain exhibit on their surface several highly developed layers of epithelium—the common finely granulated epithelial cells with nucleus, in various stages of development; spherical bodies with few granules and a nucleus; spherical bodies with a nucleus but without granules, with a very thin membrane; vesicles of a considerably greater size without either granules or nucleus, transparent like water, with a very delicate, scarcely perceptible membrane, visibly dissolving under the microscope by imbibing more water—the latter are only detected soon after death, perishing almost immediately in consequence of their delicate nature. This may be considered as the cause of their not sooner having been described in man; but it is also just that changeable nature which must make us conclude that they serve the purpose of chemical transformation of the liquor sanguinis. The liquor cerebro-spinalis is, therefore, according to Luschka's view, the product of this continually growing and perishing process of these cellular bodies. Also on the surface of the pleura, peritoneum, and pericardium, besides the usual epithelial cells, others are found (immediately after death) analogous to the transparent, easily perishing bodies just described; and a similar formation is met with in the membrana hyaloidea.

3. DONDERS could not detect the *hepatic cells* in the bile even of the smallest gall-ducts, as described by Wharton Jones; but in the secretion of the *pancreas* and *salivary glands* he always found some glandular cells. The *mucus* is considered as the product of the epithelial cells which may communicate their contents to the surrounding fluid as well by bursting as by endosmosis and exosmosis.

4. Dr. SIEGMUND'S paper will be analyzed in another place. We, therefore, limit ourselves here to mention that he comes by his experiments (on rabbits) to the conclusion that the abnormal increase of urea, whether produced by digitaline or by action on the vagus, is the consequence of a constitutional condition analogous to what is generally called "fever" (pyrexia) without any local affection of the kidneys or specific influence on these organs.

5. Dr. GOLL made his experiments on large dogs. The collection of the urine was performed by opening the abdominal cavity on both sides, and attaching thin glass canulas to the ureters. In all the observations the urine was collected during at least twenty minutes, in general, however, during thirty to forty-five minutes. The pressure of the blood was measured in some experiments, according to Volkmann's method, by the "kymographion;" in others by the "planimeter" of Wetti.* The pressure of the blood was varied: 1. By irritation of the *n. vagus*, the experiments of Ludwig and Hoffa† having proved that by irritation of the vagus the pressure is diminished. Goll is, however, aware that by this method, besides the heart, also the brain and the other nervous system may be influenced, and, through these, the secretion of the urine. 2. By *venesection* and subsequent *re-injection* of the defibrinated blood. 3. By ligature of the carotids, the cervicales ascendentes, and in one case also the iliacæ communes.

The result of these experiments was, that during the state of irritation of the vagi, and while the quantity of the circulating blood was diminished (venesection), the quantity of urine was lessened, while it was increased by ligature of the arteries. At the same time, Goll made the curious observation, that the quantity of urine secreted by the one kidney was scarcely ever equal to that of the other kidney, nor did any proportion exist between them, but the same kidney secreted now more and then less than the other; and yet the pressure of the blood and its composition (the two factors generally considered as most important) must have been altered simultaneously, and in an equal manner for both kidneys. We cannot help, therefore, assuming the existence of other unknown conditions, influencing the secre-

* Transactions of the Imperial Academy of Vienna, p. 134, 1850.
† Henle und Pfeuffer's Zeitsch. für rat. Med., Band ix. pp. 124, ss.

tion, which may be looked for perhaps in the muscular coats of the smallest arteries, in the capillaries, and in the walls of the urinary tubules. At the same time, however, we must conclude that, besides these unknown circumstances, the pressure of the blood in the arterial system exercises an important influence on the secretion of the urine.

6. Dr. Homolle bases his inferences on the changes which he observed in the *urine after bathing*, and principally in its reaction and specific gravity. The bath consisted, in part of the experiments, of mere water; in another part, of a solution of mineral salts and organic alkaloids. In another series of experiments, the endosmotic power of the skin, deprived of fat, is compared with that of the membranes of the intestinal tract. The chief inferences to which Homolle is led are, that water is absorbed through the skin during bathing; that the mineral salts are decomposed before entering into the system through the skin, only some of their bases being absorbed, while they pass unchanged through the intestinal membranes. The supposition, however, that the increased quantity and diminished specific gravity of the urine after bathing are attributable only to absorption of water, is not proved by the author in his present essay.

Homolle further remarks:—1. That even the urine which had been acid before bathing, becomes alkaline after a bath. 2. That the urine becomes more alkaline after a bath with sodæ murias, potass. iodid., and potass. ferro-cyanide, than after a bath with potass. carbon. 3. If potassa is the base of the mineral salt in solution, the quantity of potassa in the urine becomes increased. 4. This increase of potassa is in combination with carbonic acid. 5. No iodine is found in the urine after a bath with potass. iodid. 6. The chlorides of the urine are not increased after a bath with seasalt or ammonia murias. 7. Even after a bath with acid salts the urine becomes alkaline. 8. No other bases but soda and potassa enter into the urine. 9. Organic substances, added to the bath, exercise no influence on the constitution.

V. Nervous System.

1. *Means for Examining the State of the Nervous System.* By Dr. Miquel. (Arch. des Vereins für Gemeins. Arbeiten. No. III., pp. 386 ss. 1853.)
2. *On the Nerves of the Vessels of the Stomach and the Function of the Middle Fasciculi of the Spinal Marrow.* By Dr. Schiff. (Vierordt's Arch. für Phys. Heilk. Band xiii. pp. 31 ss. 1854.)
3. *On the Accommodation of the Eye.* By Prof. L. Fick. (Müller's Arch. p. 449. 1853.)
4. *On the Relation of the Invisible Rays of high Refrangibility to the Media of the Eye.* (Müller's Arch., p. 459. 1853.)
5. *The Doctrine of the Sense of Touch.* By Dr. G. Meissner. (Henle's and Pfeüffer's Zeits. für rat. Med., Band iv. Heft 2. 1853.)

1. The means proposed by Dr. Miquel consist in the application of various degrees of pressure to the stem of a nerve, during a longer or shorter space of time. Dr. Miquel found that the feelings of numbness, formication, pain, &c., produced by pressure, appear sooner, are more intense, and last longer, when the nervous system is in a less vigorous state than otherwise. He states that in the same subject, and on the same day, the numbness could be produced sooner in the evening (already after about three minutes) than in the morning (after six to fifteen minutes), and that whenever the right arm was more used than the left, a remarkable difference was observed in favour of the arm less used. Miquel made his experiments on the *nervus ischiat.* and *nervus medianus.* To the former the pressure was applied by leaning the weight of the whole body on the spot, where it passes between trochant. maj. and tuber isch. to the posterior part of the thigh; to the latter, by compressing it on the external side of the arter. brachialis. Dr. Miquel invites inquiry into these methods, which, if made more accurate,

might, he thinks, become useful as well in a merely physiological as also in a pathological point of view.

2. Dr. Schiff, experimenting on rabbits kept under the influence of chloroform, found that the motions of the stomach usually caused by irritation of the thalami optici, the pedunculi ad cerebrum and cerebellum, the pons and the hemispheres of the cerebellum are impaired by the section of the nervi vagi, while this operation is not followed by any alteration in the structure of the membrana mucosa. Dr. Schiff concludes from these facts that the vagi contain almost only the motor nerves for the stomach, scarcely any trophic nerves. The thalami optici and crura ad cerebrum are connected with the nervous fibres for the vessels of the stomach (probably through the sympathicus), as section of the thalamus opticus, or the crus cerebri of either side, is followed, after eight days, by stagnation of the blood and softening of the mucous membrane, and after fourteen or eighteen days, by partial softening of all the membranes, with exception of the peritoneum. Section of the spinal marrow *below* the origin of the nervi vagi has the same effect as that *above* their origin. Section of one half gives rise to the same changes as that of both halves of the spinal marrow. The same is the case with the section of merely the anterior fasciculi. Section of the *middle* fasciculi has no influence on the nutrition of the stomach. Dr. Schiff does, therefore, not adopt the opinion of Bellingeri, that the middle fasciculi contain the vegetative nerves of the stomach; but they are intimately connected with the respiratory functions, as their section produces paralysis of the respiratory muscles of the corresponding side, while a similar result is not observed either after section of the anterior or after that of the posterior fasciculi. Dr. Schiff's experiments are therefore in accordance with those of Valentin and Longet, and with the theory proposed by Sir Charles Bell.

3. Fick maintains the view now almost generally adopted, that the mechanism of the accommodation is to be found in the alteration of the form and position of the lens. He considers the *uvea* to be the apparatus which effects, besides the regulation of the quantity of light (through the motion of the pupil), the accommodation for near and remote objects, by means of a change of the position and form of the lens, placing varying quantities of blood now before then behind the lens.

The facts on which this view is based are the following:—1. The very limited connexion between the vessels of the choroidea and the iris. 2. The facility with which, by injection from the single venæ vorticosæ and ciliary arteries of the choroidea, a considerable part of the less minute arteries and veins, forming the larger meshes of the choroidea, and a corresponding portion of the processus ciliares, is filled,—while the capillary layer of the choroidea (membr. Ruyschiana) is filled only by increased force, the vessels of the iris remaining empty. 3. The vascular arrangement in the choroidea and processus ciliares exhibits no analogy with the vascular apparatus of other organs destined for their nutrition, exhibiting scarcely any capillary system except that of the membr. Ruyschiana, but an almost immediate transition from the arteries into the veins. 4. The perfectly normal state of the vascular apparatus in the uvea of Albino's demonstrates that the production of pigmentum can certainly not be the only function of this apparatus. 5. The existence of so strong a junction between the capsula lentis and the choroidea where the prolonged folds of the choroidea project into the posterior chamber of the eye. 6. The *contractility* of the *processus ciliares*, which L. Fick assumes as well from experiments performed with A. Fick (having produced contraction of the congested ciliary process by means of electricity), as also from the circumstance of the processus ciliares being, after death, always found empty, except after death by strangulation,—as also from pathological facts. 7. The peripheric part of the lens is softer than the central.

Fick sees in the *erectile* and *contractile* vessels of the processus ciliares an appa-

ratus by which, the form of the bulbus remaining unchangeable, in the state of impletion of the vessels, pressure is exercised on the anterior capsula of the lens, while through their depletion into the venæ vorticosæ a pressure is exercised on the posterior capsula, effecting the lens slightly to advance towards the anterior chamber, and to become at the same time rather more convex—a process in which the difference in the consistency of the fluids of both chambers is likewise of importance.

4. DONDERS applied the discoveries of Sir John Herschel, Brewster, and principally Stokes,* that a solution of the sulphas quinæ makes the invisible rays of high refrangibility lying beyond the violet (the so-called "chemical" rays) appear in a blue light, to the investigation of the relation of these rays to the media of the eye. It is well known that Brücke† had lately concluded (from his experiments with tincture of guaiacum) that these chemical rays are absorbed by the media of the eye, that they are invisible on account of their being prevented from reaching the retina. Donders placing the transparent media of the eye in the way of the invisible rays of high refrangibility, and behind the media of the eye a paper screen painted over with a solution of sulphas quinæ, found the latter appear almost as intensely blue as if the media of the eye had not been in the way of the rays. Brucke's view, that the cause of the invisibility lies in the media of the eye, can after this not be adopted, and it appears, therefore, that the human retina itself is insensible for these chemical rays.

5. MEISSNER endeavours to prove in the present (as already in a former‡) essay, principally by theoretical remarks, that the sense of touch is a special sense, that the specific perception of this sense consists "in a psychical state of excitation, the essential and distinctive property of which is that it possesses in itself a compelling reason for the soul to form an idea of an external object in contrast to anything belonging to our own body." The sensation of touch appears to Meissner not identical with that of pressure. The so-called sensory papillæ are, according to this view, the specific organs for the sense of touch, in the same manner as the retina is the organ for the sense of sight.

QUARTERLY REPORT ON SURGERY.

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I. OPERATIONS.

1. *Rhinoplasty.* By M. BAUDENS. (Bull. Gen. de Thér., tom. xlvi., p. 262.)
2. *Plastic Operation.* By Dr. CARNOCHAN. (American Medical Monthly, Jan., 1854.)
3. *New Amygdalotome.* By M. MAISONNEUVE. (Bull. Gen. de Thér., tom. xlvi., p. 219.)
4. *Autoplastic Operation for Cancer of the Rectum.* By M. DEMARQUAY. (Bull. Gen. de Thér., tom. xlvi., p. 41.)
5. *On the Treatment of Periodic Hæmorrhage after Operations.* By M. BOUISSON. (Bull. Gen. de Thér., tom. xlvi., pp. 12 and 102.)

1. M. BAUDENS has put in practice a happy idea in rhinoplasty—viz., that of saving the cartilages of the nose, if possible. He prefers taking the flaps for the new nose from the cheeks and sides of the old one; and in an example of cancer of

* Phil. Trans., 1845 & 1852; Poggend. Annal. Supplement., vol. iv. 2.
† Müller's Archiv, 1845 & 1846.
‡ G. Meissner: Beitraege zur Physiologie der Haut. Leipzig, 1853.

the end of the nose, which appears to have affected the skin and subjacent tissue, without involving the cartilages, he has saved these latter, and so has preserved an efficient support for the flaps of skin afterwards made to cover them. The preservation of the nasal cartilages wherever practicable is, of course, a point gained in the subsequent preservation of the form of the nose, and the prevention of the too common flattening of the newly-formed part.

2. A very bold and successful attempt at restoration of the upper lip has been made by Dr. CARNOCHAN, of New York; an operation which is rarely called for in comparison with those on the lower lip. The patient was a lady, aged 39. When about 22 years of age, a pimple appeared on the upper lip, which enlarged, ulcerated, was considered cancerous, and removed by operation. She remained free from disease for 8 or 9 years, when the cicatrix became swollen and again ulcerated; and, with some interruptions, the disease progressed until April, 1853. At that time the whole thickness of the upper lip was destroyed up to the nose. Nearly its whole length was ulcerated; the right ala nasi was detached. Edges of ulcer hard; no glands enlarged. Two quadrilateral flaps were made in the cheeks, by incisions passing outwards from the base of the nose, and from the angle of the mouth as far as the masseter muscle. The diseased edges were all cut off, and the flaps being detached on their under surface were made to slide over the jaw, and meet in the middle line. A great number of pins were used. The cure was rapid, and the deformity left behind very slight. So complete a change was effected in the general health and comfort of the patient, that she felt as if she "inhabited another body."

3. An ingenious instrument for seizing and excising a portion of the tonsil, manageable by one hand, has been invented by M. MAISONNEUVE. It consists of a modification of the tonsil-guillotine, and the adaptation of a sort of fork, by which the redundant part of the tonsil is transfixed from before backwards, and brought away, after it is cut off by a ring-shaped blade, which moves from behind forwards. It would be impossible to convey a correct idea of the instrument without a figure.

4. In a recent case of rectal cancer in a female, M. DEMARQUAY removed the lower part of the rectum all round, and then, having dissected up the healthy edge of the bowel, he drew it down, and attached it to the skin at the margin of the anus. The wound was thus much diminished in extent. A supple cicatrix was the result, and the function of the part was quite restored.

5. M. BOUISSON, of Montpellier, has brought prominently forward the fact that distinctly periodic losses of blood are occasionally met with as one of the forms of hæmorrhage after surgical operations; and that this intermittent form of hæmorrhage must be carefully distinguished from other kinds dependent on local accidents, or conditions of the wound, on particular states of the blood or circulating system, on arrest of any ordinary discharge, either catamenial or hæmorrhoidal, or on physical or moral excesses. The previous abode of the patient, in a malarious locality, the regular periodic character of the hæmorrhage, the co-existence of a certain amount of febrile disturbance, and the occurrence of the bleeding at the *crisis* of the attack (as it were, as the substitute for sweating), are the principal grounds on which we may diagnosticate this form of consecutive hæmorrhage. In certain cases all these conditions are fulfilled; but in a few, M. Bouisson believes that the intermittent character only is to be detected, there being no regular febrile access, just as we observe in the masked fevers, or *fièvres larvées*. Finally, the most interesting fact connected with these periodic hæmorrhages is, that they yield positively and promptly to full doses of quinine, even when other internal treatment, as by astringents, has apparently failed

M. Bouisson remarks that this periodic hæmorrhage from *wounds* after opera-

tions, has received little attention from surgical observers, probably owing to its rarity, except in localities prone to the development of malarious influences. In his special "treatise on periodic diseases without fever," M. Casimir has mentioned periodic bleedings from mucous surfaces, but not from wounds. Professor Sanson has briefly alluded to periodic secondary hæmorrhages, deriving his knowledge also from observations in Montpellier. Besides this nothing but scattered allusions to the subject exist in surgical literature.

The special cases brought forward by M. Bouisson may be thus analyzed. They are four in number—viz., an amputation of the great toe, in a girl 18 years of age; amputation of the leg in a male aged 36 years; amputation of the thigh in a male of 21 years; and removal of the forefinger, with its metacarpal bone, for encephaloid disease of the latter, in a female aged 34.

As to the influence of locality, the first patient came from the neighbourhood of Arles, in which intermittent fevers are so common. The second was placed in a particular ward of the Hospital Saint Eloi, in which intermittent fever often made its appearance amongst the patients. No special local circumstances could be traced in the third and fourth cases.

The periods at which the hæmorrhage began in each case, the number and dates of its recurrences, and the mode in which it was apparently affected by the administration of quinine, were as follow. In the first case, it began on the evening of the fifth day after the operation, before any ligatures had come away; it recurred at the *same hour* on the sixth and seventh days; during the early part of the eighth day, 10 grains of the sulphate of quinine were given, and the bleeding never recurred. In the second case, the hæmorrhage first happened on the ninth day after amputation, no ligature being loose; it recurred at the same hour on the following day; on the eleventh day, 12 grains of quinine having been irregularly given, it again happened at the usual hour, but in more moderate quantity; on the next day, the remedy being persevered in, it ceased. In the third case, the hæmorrhage began on the evening of the eleventh day, and recurred at the same hour on the thirteenth and fifteenth days, missing the intermediate days. It was diminished on the fifteenth day by 10 grains of sulphate of quinine, and arrested entirely by its repetition on the seventeenth day. Powerful astringents had been first tried. In the fourth and last case, the first hæmorrhage took place at seven o'clock in the morning of the fourth day after the operation; it was renewed at precisely the same hour on the fifth. Quinine being administered, it did not happen on the sixth day; but the patient having then neglected to take that medicine, a fresh bleeding happened at seven in the morning of the seventh day; but by subsequent attention and an increase of dose, further bleeding was arrested. In all four cases, great care was taken to determine that the hæmorrhage was a general oozing, and not from any particular vessel. Sometimes even compression was employed, but without success. In all cases, the quinine seemed alone to arrest the hæmorrhagic attack; and its use was invariably continued for some days after the bleeding had ceased.

As to the general symptoms accompanying the attacks of bleeding, they were, in the first and second, well-marked, consisting of a cold and then a hot stage, at the height of which the bleeding occurred. In the third case there was no cold stage, but merely a slight febrile accession. In the fourth example no general disturbance at all was manifest: the remarkable periodicity of the attacks alone serving as a guide in the diagnosis and treatment.

M. Bouisson refers generally to other cases, collected from the clinical records of Montpellier. It is proper to add that he by no means neglects the ordinary rules of treatment in cases of consecutive hæmorrhage. That an intermittent or periodic consecutive hæmorrhage from the general surface of a stump occasionally happens—that its cause is constitutional and similar to that which gives rise to intermittent fever—and that it yields promptly to quinine—are conclusions to which M. Bouisson's observations irresistibly lead. Whether the bleeding is to be regarded, as he believes, as the substitute for the ordinary critical discharge—

of sweating—and whether a periodic hæmorrhage without febrile disturbance is truly analogous to the masked fever, are points perhaps for further investigation. We commend the subject to our surgical friends living in malarious districts.

II. Affections of the Genital Organs.

1. *On Stricture of the Urethra.* By M. Maisonneuve. (Bull. Gén. de Thér., tom. xlvi., p. 169.)
2. *The Treatment of Nocturnal Emissions.* By M. Laroche. (Ibid., tom. xlvi., p. 76.)
3. *Varicose Veins of the Labia Majora.* By M. Morpain. (Ibid., tom. xlvi., p. 160.)

1. M. Maisonneuve has lately employed a mode of dividing strictures of the urethra from *within outwards,* which differs but slightly in principle from methods already in use. The instrument, however, differs materially. It consists of a *bistouri caché,* or of the lithotomy knife of Frère Côme, somewhat modified to suit its new office. The new urethrotomy knife is longer, more slender, and bent sideways; the sheath is smaller and cylindrical, especially at its free end. The hinge and handle are so arranged that, with the thumb, the blade can be projected from the side at pleasure. The instrument is introduced through the stricture with the blade concealed, and is then withdrawn with the blade projected, so as thus to divide the walls of the urethra at the seat of stricture. The incision is recommended to be lateral, and to be made on both sides.

It will be seen that this operation resembles very closely in principle the numerous improved methods of dividing the stricture internally, which followed on the publication of Mr. Stafford's paper on the 'Lancet-stilettes;' methods, we mean, in which the urethral walls are divided by a cutting blade, or blades, projected from a tubular instrument, and so withdrawn after this latter has been passed through the stricture. These M. Maisonneuve calls urethrotomy from *behind forwards,* and his own method from *within outwards.* Certainly, the distinction appears to us more verbal than real; and the cutting stilette is as neat and manageable as the new urethrotome.

M. Maisonneuve insists on the value of this mode of dividing a succession of constrictions, and remarks that perineal abscess does not follow the operation if the incisions be lateral, and not on the middle line downwards.

2. M. Laroche relates the case of a youth, æt. 18, whose nightly pollution was instantly stopped on taking a dose of digitaline equal to three grains of the powder of the leaves. The remedy was continued (more or less?) for forty-five days. The symptoms returned only twice—viz., on the twelfth and thirtieth nights after the treatment began. MM. Corvisart and Brughtmans also advise this application of digitaline.

3. An enlarged or varicose condition of the veins of the labia majora passing up to the inguinal ring in the female, has been described by M. Morpain under the title of *varicocele in the female.* This is not the place to discuss the anatomical questions which this appellation would suggest, but certainly one may say that the spermatic veins in the male are those chiefly concerned in varicocele in that sex, so that the condition above described in the female is not strictly analogous to male varicocele. The affection, however, exists, arising generally during pregnancy, and is occasionally very troublesome. M. Morpain has now recorded two cases, one treated successfully by M. Debout, by mercurial inunctions (!) and pressure; the other by Huguier, who employed Vidal's method of ligaturing, or constricting the veins by means of two fine wires of brass, which are passed on each side of the vessels, and are then twisted together at their ends. Three such ligatures were required.

III. Affections of the Rectum.

On Imperforate Anus. By Dr. Chevers. (Indian Annals, No. I., p. 296.)

Dr. Norman Chevers, in a short paper, full of matter, discusses the varieties of this most intractable deformity in the male infant, and gives the particulars of two fatal cases in which the intestine communicated with the urinary passages. Considered morphologically, the classification which Dr. Chevers has adopted is imperfect, but practically it is useful. In the *one set* of cases, the only defect is a contraction or obliteration of the anal orifice: the treatment of these is simple, obvious, and generally successful. In *another set* of cases, the rectum, or pelvic portion of the alimentary canal, is absent; there is no anus; and the colon, ending in a cul-de-sac opposite the sacral promontory, may or may not communicate with the urinary passages either at the bladder, or frequently at the membranous part of the urethra. An occasional escape of meconium by the urethra will of course disclose such a communication, but a communication may exist without such an escape. An examination with a probe (per urethram) should always be made, to determine the question, if possible, for thus we may obtain a guide to reach the distended gut. Where such a communication exists, however, the termination of the case is usually fatal; where it does not, the operation may be successful. In the performance of this, Dr. Chevers recommends the use of a half inch trocar, as being neither too large nor too small, and that it should be passed up boldly through a perineal incision in the middle line, as high as the promontory of the sacrum, where the distended gut is most likely to be met with. In a *third* set of cases, the obliteration may affect any part of the intestinal canal, from the duodenum to the sigmoid flexure; whilst the anus and pelvic part of the rectum are very much contracted. A probe passed up is soon arrested, and bougies merely dilate the bowel, and admit of an examination by the fingers. It is necessary, in such cases, to decide whether an operation should be done by puncture from the dilated pouch, or, for artificial anus, over the seat of the colon. The position of the distension in the abdomen is one guide to the seat of the abnormal obstruction. Distension may not be felt per rectum, although the distended bowel be the colon at the sacral promontory: an operation per rectum should therefore be tried first, if the lower part of the abdomen be much distended.

In all cases, the operator should wait for the lowest pervious part of the bowel to be fully dilated; but no longer than when vomiting has set in.

QUARTERLY REPORT ON MIDWIFERY.

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I. Diseases of the Unimpregnated Uterus. Ovarian Disease.

1. *The Surgical Treatment of certain Fibrous Tumours of the Uterus, heretofore considered beyond the resources of Art.* By Washington L. Atlee, M.D. (Trans. of Amer. Med. Assoc. 1853.)
2. *The Pathological Changes in the Mucous Membrane of the Uterus, &c.* By Dr. C. Mayer. (Verhandl. der Gesel. für Geburtshülfe in Berlin. 1853.)
3. *A Successful Case of Ovariotomy.* By Dr. Gurlt. (Verhandl. der Gesel. für Geburtshülfe. 1853.)
4. *A Successful Case of Ovariotomy.* By Dr. Bradford. (Amer. Journ. of Med. Sc. Aso. 1854.)

1. Dr. Washington L. Atlee has, in a prize essay, published in the 'Transactions of the American Medical Society,' put forth some rather bold innovations in

the surgical treatment of fibrous tumours of the uterus. He classifies these tumours according to their situation, into extra-uterine, intra-uterine, and intra-mural. He does not appear to regard tumours of any kind or situation to be beyond the reach of surgical treatment. He believes that the true fibrous tumour occasionally degenerates into cancerous disease.

One of Dr. Atlee's principles of treatment is based upon the following view: "These tumours are very imperfectly organized; consequently their vitality may be very easily destroyed. A section made through their thin investing membrane will sometimes be followed by the death of the whole mass. This may be owing to the admission of atmospheric air causing it to degenerate. Indeed, it would appear that the action of the oxygen of the air, like a portion of yeast in a fermentable mass, may originate in any part of a fibrous tumour, an action of eremacausis which may extend throughout the whole."

Another mode of treatment is thus stated: "The excessive hæmorrhages which sometimes occur, arise not from the uterus itself, but from the vessels of the membrane which covers the tumours. These floodings, I think, occur in this way: the veins of the investing membrane become at times greatly engorged, in consequence of their circulation being impeded by the muscular action of the uterus, while the arteries, by reason of their more resisting coats, continue to supply them with blood. The point of least resistance must necessarily be at the os uteri, as all other parts are compressed by the contracting uterus. The veins on the surface are thus distended. The mucous membrane is delicate, and offers but little resistance to the rupture of these vessels. Now the practice which I wish to inculcate, as based upon the above fact, and which has invariably arrested hæmorrhage instantaneously, is, *during hæmorrhage, to pass the bistoury along the vagina into the cavity of the uterus, and make a very free incision into the most exposed portion of the tumour.*"

As the most comprehensive way of conveying some idea of the operative proceedings of Dr. Atlee, and their results, we extract the headings of the cases related, with brief remarks in illustration.

CASE 1. "Mrs. M., 49 years old; tumour intra-uterine, nearly its entire surface sealed to the interior of the uterus, even down to the edge of the os tincæ; *the whole tumour removed;* supposed weight, ten pounds; recovered; death subsequently from inflammation of the lungs." The plan resorted to, was by successive operations to separate the adhesions, and to force the tumour lower into the pelvis by ergot. Portions of the tumour were then cut off by the bistoury. The next step was to bring away pendulous portions with the cranial perforator. After persisting in this course for a considerable time, the uterus being supported externally, Dr. Atlee "succeeded in breaking up the whole internal structure of the tumour, and in scooping out a large quantity of it." This proceeding was repeated some days after Every one will share in the regret expressed by the author, "that an imprudent exposure to cold, and a subsequent alarm, interfered with the recovery of the patient, by establishing a fatal disease in the lungs at the very moment when the patient herself, her friends, and the surgeons were congratulating themselves on the successful issue of this unique case."

CASE 2. "Mrs. J. M., aged 49; tumour intra-mural, having been developed in the posterior wall of the uterus, and expanding that wall into a cyst inclosing it; *the whole tumour removed;* supposed weight, four or five pounds; recovered." A sketch illustrating this case represents the tumour very much larger than the uterus itself. A similar treatment by ergot and incisions through the tumour was adopted. Dr. Atlee remarks, that although a long and deep section of the tumour and its coating was made, no hæmorrhage followed, notwithstanding severe floodings had previously occurred. The operation attempted was that of enucleation; but the greater portion of the mass gradually wasted away by a species of decomposition.

CASE 3. "Mrs. J. M'B, aged 30; tumour intra-uterine; os tincæ thick and closed; whole tumour removed; supposed weight, six or seven pounds; recovered. Subsequent reproduction of the tumour; again removed; recovered."

CASE 4. "Miss M. T., aged 33, tumour intra-uterine, its entire surface intimately incorporated with the interior of the uterus; *its removal attempted by gastrotomy,* which failed; subsequent recovery, and an attempt made to remove it per vias naturales; death from erysipelas."

CASE 5. "Miss M. B., aged 36; tumour intra-uterine, and distended the uterus to the size of full pregnancy; os tincæ closed; cervix entire and dense; orifice very small; the whole tumour removed; supposed weight eight or nine pounds; recovered." Ergot, detachment of the adhesions, and cutting into the substance of the tumour so as to induce decomposition, were the means employed. The cervix uteri was first incised so as to facilitate its expansion.

CASE 6. "Mrs. S. B. K., aged 42; tumour intra-mural; was developed in the posterior wall of the cervix, expanding it into the form of a cyst; occupied the abdomen to the height of the umbilicus; patient bloodless from repeated floodings, and her life in imminent hazard from present hæmorrhage; bleeding ceased immediately on operating, (a long bistoury was introduced into the cavity of the uterus, the edge turned backward upon the tumour, the posterior wall of the cervix and os uteri, down through the corresponding wall of the vagina which formed the antero-inferior covering of the tumour cut through, the tumour incised, enucleation;) *removed the whole mass, weighing nine or ten pounds, at once through the os externum.* Death from anæmia."

CASE 7. "Miss A. B., aged 49; tumour intra-uterine, and sealed to the interior surface of the uterus, extending to within one inch of the umbilicus; the cervix was lost in the tumour, and the os was firm and ring-like; removed one-third of the tumour; died suddenly from disease of the heart." Operation: ergot, detachment of adhesions, incisions into tumour, incisions of os uteri, crotchet to aid in disintegrating tumour.

CASE 8. "Miss H. B., aged 31; tumour intra-uterine, and sealed to the interior of the uterus; very prominent, and extended above the umbilicus; cervix entire and moveable on the tumour; os tincæ closed; supposed weight seven or eight pounds; recovered." Operation: os and cervix opened by the knife; the tumour incised, degeneration (sloughing) induced; ergot.

CASE 9. "Mrs. E. B., aged 36; tumour intra-mural; cervix uteri bent against the tumour at an acute angle; operative measures (ergot, incisions of os and cervix, and into tumour) discontinued before the tumour entirely disappeared; recovered."

CASE 10. "Miss E. K., aged 35; tumour intra-mural, very prominent above the pubis, extends upwards within 2½ inches of the umbilicus; cervix folded up against the tumour; tumour as large as a child's head; removed in detached portions; apparent convalescence; death from peritonitis; disease malignant." Operation: ergot, incision into tumour, partial enucleation, putrefaction induced.

CASE 11. "Mrs. E. W., 47 years; tumour intra-mural; the whole anterior wall nodulated from the fundus to the os tincæ; patient perfectly anæmic; incised the whole length of the uterus; recovered." In this case, the removal of the tumour does not appear to have been undertaken: the incision was intended to arrest the hæmorrhage, which it is said to have accomplished.

CASE 12. "Mrs. E. A. M., aged 42; tumour extra-uterine or pelvic; the uterus and bladder raised into the abdomen; *gastrotomy;* non-removal of the tumour; recovery from the operation; subsequent operations per *vias naturales;* tumour diminished in size; recovered." The subsequent operations were—incisions into the tumour, partial enucleation, setting-up of *eremacausis.*

CASE 13. "Mrs. W. G., aged 62; tumour intra-uterine; attached to cervix; tumour removed; recovered *from the operation;* apprehension of cancerous degeneration."

CASE 14. "Mrs. S. G., aged 49; tumour intra-mural; having been developed in the anterior wall of the uterus, and expanding that wall into a cyst enclosing it; *the whole tumour removed;* supposed weight, seven or eight pounds; recovered."

Operation: ergot; incisions into tumour; partial enucleation; removal of portions by crotchet and forceps; induction of eremacausis in remainder.

2. Dr. Mayer, after adverting to the statement of Dr. Robert Lee, that out of 1000 dissections of women, only 20 exhibited disease of the uterus, and to the similar statements of Messrs. Hewitt, Pollock, and Gray, remarks, that Virchow has recently turned his attention to the morbid alterations of the uterine mucous membrane in the dead body, and has found them of frequent occurrence. Dr. Mayer observes that, in the living body, these pathological changes are remarkably common; that they are found at every age, in children, virgins, young and old women, in the barren, and in those who have borne children. The various diseases of the uterus are the constant companions of nervous affections of every kind; and it may be asserted that very few hysterical females are free from uterine affections—that is, diseases of the mucous membrane, erosions, and the further development of these into ulcerations. In the last two years and a half, Dr. Mayer has met with 14 retroversions, 65 *retroflexions*, 44 anteversions, and 51 anteflexions of the womb; 21 polypi, and 26 ovarian tumours.

In another communication (p. 79), Dr. Mayer stated his opinions as to the use of the speculum. He gives the following view of the pathological conditions of the os uteri and cervical canal, that may be detected by the speculum:—I. *The catarrhal affections*, subdivided as follows: 1. Erosions, excoriations of the lips of the os uteri; 2. Chronic catarrhal ulceration of the lips and of the cervical canal; 3. The aphthous form of the lips; 4. The follicular ulceration of lips and cervical canal—diseased follicles of the mucous membrane, *ovula Nabothi*—polypi of the mucous membrane; 5. Papillary degeneration of the mucous membrane of the os and cervical canal: the hæmorrhagic form; the knobby or granular form; the fungous form, with small soft granulations; 6. The varicose ulceration with enlarged capillaries, and the telangiectatic swelling of the lips. II. *The syphilitic ulceration.* III. *The phagedænic ulceration.* In addition to these, fibrous polypi emerging from the os, the cauliflower excrescence, and scirrhous ulcerations, may also be discovered.

The author discusses the question: In what diseases of women is the speculum useful? His answer is, not only in all disturbances of the functions of the sexual organs, in irregular, scanty, profuse, or painful menstruation, in sterility, in disposition to abortion, in blennorrhœa, in painful or difficult micturition, in obstinate constipation, especially in all painful sensations in the pelvic organs, but also *in all nervous affections, in hemicrania and prosopalgia, in neuralgia of the extremities, in hyperæsthesia of the most different nervous tracts, in hysterical suffocation, in returning cardialgia and colics, and even in disorders of the mind.* The physician will thus frequently discover the right clue to a rational treatment in the numberless affections of the nervous system.

3. Dr. Gurlt relates the history of a successful case of ovariotomy by Langenbeck. A single woman, æt. 34, had borne an ovarian tumour for five years. An incision two and a half inches long was made in the *linea alba*. The cyst punctured, about nine quarts of fluid were abstracted, and the sac was drawn out. The pedicle was transfixed, and ligatures applied. The wound was closed by the knot-suture, and ice-water compresses applied. The after-treatment consisted in a bleeding on the evening of the operation, and repeated doses of morphia. The cure was completed in two months. The extirpated sac showed a thick-walled simple cyst, bigger than a man's head. It was probable that the left ovary was removed with the tumour, since several smaller cysts were found attached.

4. Dr. Bradford relates a successful instance of ovariotomy. An unmarried lady, æt. 21, had been suffering from an ovarian tumour for twelve years. The tumour was of large size, rising to the ensiform cartilage; and upon the anterior superior part above the umbilicus there was a hard, bony substance, imbedded in

the sac, and about the size of a saucer. An incision in the median line, five inches long, was made. The cyst was found strongly adhering to the omentum at the upper part; it became necessary to extend the incision four or five inches higher. The bands connecting the tumour to the omentum were very large and firm, and were inserted by several points into the bony substance. It required considerable force to break them up. The tumour, when removed, weighed forty-one pounds. The surface of the sac on the inner and front part was rugous, and studded over with innumerable small particles of bone. The ligature came away six weeks after the operation. Seven months after the operation the patient was reported in good health, having gained flesh.

Dr. Bradford states, that out of ten cases of ovarian disease examined by himself and Dr. Dunlop, they had operated in four, the result being in all these cases successful.

II. Pregnancy, and Theory of Parturition.

1. *On the Duration of Pregnancy, the Causes of Birth, and the Mode of Uterine Contraction.* By Dr. Gustav. Veit. (Verhandl. der Ges. für Geb.—*Berlin*, 1853.)
2. *Three Cases of Albuminuria during Pregnancy.* By Dr. Hecker. (*Op. cit.*)
3. *Cases of Pregnancy attending Anomalous formation of the Uterus.* By Prof. L. Chiari (Vierteljahrsch. für das prakt. Heilkunde. 11 Jahrg., 1854. Zweiter Band.)

1. Dr. Gustav. Veit has communicated an elaborate essay upon the duration of gestation, the causes of labour, and the mode of uterine contraction.

1. *On the Duration of Pregnancy.*—It is admitted that the period of fructification of the ovum is the only *terminus a quo* which can serve for the precise reckoning of gestation. We can at most determine the period of the fruitful coïtus; never of the fructification itself. We have good reason to conclude that between these two periods, not hours only, but days, may intervene.

The author has tabulated 45 cases from Reid, Montgomery, Girdwood, Rigby, Lockwood, Lee, Desormeaux, Dewees, Beatty, Skey, M'Ilvain, Ashwell, Cederschjöld, and others, in which the date of impregnation appeared to be fixed by a single coitus. From this table it appears that the shortest pregnancy was one of 263 days, and the longest 300 days, leaving a variation of 37 days. Labour came on 8 times between the 263rd and 270th days; 25 times between the 271st and 280th; 7 times between the 281st and 290th; and 5 times between the 291st and 300th days. The mean was 276·93. It is obvious that these results can only furnish untrustworthy and uncertain conclusions, from the limited number of observations.

The author next examines the evidence derived from the observations of Tessier, Lord Spencer, and Krahmer, upon the duration of gestation in animals. He observes in these also a great variation as to the time. That males are carried somewhat longer than females results from the observations of Spencer and Krahmer.

The impossibility of determining the date of the fruitful intercourse has driven obstetric practitioners and women to estimate from the day of the last menstruation. This method has of late received a sanction from the observation, that an ovum ripens and escapes from the ovary at this time, if not exactly at the end of menstruation, at any rate in the latter half of its duration. (Bischoff.) It is said: "The period when menstruation should have returned for the tenth time is the period of labour;" and since 28 days is stated to be normal menstruation-type, the 280th day from the last appearance of the menses is given as the date of labour. Precise observations upon this point are yet very deficient in number. Tabulating those of Reid, Merriman, and Cederschjöld, it appears that out of 757 cases, 14, or 1·85 per cent., labours took place in the 36th week; 27, or 3·56 per

cent., in the 37th; 72, or 9·51 per cent., in the 38th; 117, or 15·45 per cent., in the 39th; 202, or 26·68 per cent., in the 40th; 167, or 22·06 per cent., in the 41st; 98, or 12·94 per cent, in the 42nd; 40, or 5·28 per cent., in the 43rd; 14, or 1·85 per cent., in the 44th; 5, or 0·66 per cent., in the 45th; and 1, or 0·13 per cent., in the 46th week.

In the interval of four weeks between the 267th and 294th days, only 77·13 per cent. of all the births took place; whilst the extreme limits were the 248th and 316th days,—a range of 68 days.

Devilliers found a mean of 274 days; Merriman, of 280; Cederschjöld, of 276; Reid, of 278·8; but of the total of the observations collected by the author, the number 278·5 is obtained. This number differs from the mean obtained from the observations of cases of single coitus—viz., 276·93—by 1·57 only. This would appear to support the opinion that conception follows shortly upon a fruitful menstruation.

Another question is, as to the relation between the menstrual periods and the duration of pregnancy. The law that the duration of pregnancy is a multiple of the menstrual period has, until recently, only been applied to the normal type of 28 days: Cederschjold, however, lays it down that the duration of pregnancy is governed by the duration of the *individual* menstrual period, and that the first is always ten times the latter. Cederschjold found that the greatest number of births took place between the 270th and 290th days, but that on the intermediate days few births occurred. The key to this appeared to be discovered in the frequent variation of the menstruation-cycle between 27 and 29 days. He observed that, in women whose menstrual period bore the 28 days type, the duration of pregnancy was 280 days; and that in one of the 34 days type, pregnancy lasted quite three or four weeks over 280 days, and that, moreover, in this last case, the menstruation-period and gestation terminated together. Experience, however, does not bear out Cederschjold's law. Berthold observed, in 7 cases, that whereas, multiplying the menstruation-cycle by 10, he obtained 285, 291, 295, 298, 301, 303, and 305 days, labour came on on the 273rd, 279th, 284th, 286th, 287th, 290th, and 291st days.

Krahmer's observations, moreover, do not bear out the opinion that there is a constant relation between the heat-periods and gestation-periods of animals.

Dr. Veit concludes that actual experience does not supply a definite answer to any of the questions relating to the duration of pregnancy.

II. *On the Cause of Labour.*—The greater part of the researches hitherto published have only an historical interest. The cause has been sought in one of three things:—1. In the pressure of the presenting part of the child, and the stretching of the circular muscular fibres of the uterus by the longitudinal fibres; 2. In the excessive development of the whole organ; and 3, In the menstrual congestion. The influence of the menstrual congestion is inadmissible. Kiwisch has shown that the continuation of the periodical discharge of ova during gestation is theoretically improbable, because it would be aimless; and looking to experience, the cessation of the monthly flow of blood during gestation and lactation is opposed to this doctrine. The general barrenness during lactation proves the ripening of ova ceases during this time. Moreover, in animals, the heat ceases as soon as fructification takes place, and returns only after delivery. All direct observations, again, show that in woman, during gestation, no new ovum is developed in the ovary. The author has never seen a freshly-burst follicle in the bodies of puerperal women.

He submits, that there is a period when the growth of the uterus no longer keeps pace with that of the ovum. When this harmony is so disturbed, then is there an irritation acting upon the uterine nerves, through the stretching consequent upon the expanding strength of the ovum. Analogy shows that Nature employs a similar agency in the case of other hollow organs provided with muscles: as the bladder and rectum, which are excited to contract by the quantity, not the quality, of their contents. The author approves the opinion of those who do not

place the date of the first pains in one of the last days before the end of labour, since in the last two or three weeks of gestation feeble contractions occur.

III. *On the Mode of Contraction of the Uterus.*—Kiwisch and Scanzoni represent the contractions as taking place simultaneously throughout the whole organ: Baudelocque expressed the same opinion. Wigand states that contraction begins at the neck of the uterus. Others believe that it begins at the fundus, and thence spreads over the organ. The author enters upon a rather warm criticism of the arguments of Scanzoni. He contends that Scanzoni's views as to the reflex nature of the contractions are not borne out.

If we seek to determine the question *à priori*, resort is made to experiment on animals. In these, peristaltic movements are observed, beginning at the tubal ends of the horns, and spreading in waves towards the mouth of the womb. (Valentin, Weber.) The human womb differs from that of animals only in this, that the middle portion is more developed than the horns. What does experience teach? By tactile examination, Litzmann and others have acquired the conviction that every pain begins in the fundus: most observers, however, contend that the changes wrought in the form of the uterus through the contractions cannot be perceived in the upper part sooner than in the neck. The author adheres to the latter opinion. But this, he says, does not prove that the starting-point of the contraction is not the fundus. How does the contraction act upon the uterine contents? Wigand observed that when the finger is applied to the neck when a pain came on, the presenting part of the child receded, whilst the waters came down; but Murphy and Scanzoni have shown that this does not prove the origin of pains in the neck. The moveable presenting part recedes through the pressure of the water downwards. Wimmer has observed the movements of the uterus in a case of entire prolapsus, in which labour came on at the sixth month. "It was plainly seen," he says, "that the contraction began at the fundus, and proceeded downwards to the middle part."

From the present appreciation it follows that the human womb also moves peristaltically in the direction from above downwards. It follows, also, that in every pain several waves course downwards from above, that the number of waves stands in exact proportion to the duration of the pain, and the succeeding wave begins before the first has run out. There will thus be a moment when both the upper and lower parts of the uterus will be contracting at the same time.

2. Dr. Hecker has related the histories of three cases of albuminuria in pregnant women, with and without eclampsia:

Case 1. A primipara, æt. 23, had been in good health up to the time of labour. On the evening of the 6th December she took a rich supper; she was taken ill in the night, and fell into convulsions. A period of four or six weeks was yet wanting to the term of gestation. The head presented. No œdema. The legs exhibited a perfect tetanic rigidity, as if she had been poisoned by strychnine: she foamed at the mouth, and gnashed her teeth. Immediately on the cessation of a fit some urine was drawn by a catheter. The secretion was acid, and an enormous quantity of albumen was thrown down by heat. Chloroform was exhibited, and the uterine douche employed to hasten parturition. The douche had the desired effect; but the chloroform did not in the slightest degree diminish the intensity or frequency of the fits. The patient died soon after delivery. An extravasation of blood the size of a dollar was found under the arachnoid, on the left hemisphere; and a similar one, but smaller, on the right hemisphere. The left kidney was shrunk to the third of its normal size, weighing only two ounces: it was quite unfit for the excretion of urea. The change was doubtless of long standing. The right kidney was normal.

Case 2. The eclampsia broke out after the birth of the child. A woman, æt. 31, with her second child, had a lingering labour. She had rigors. Œdema nowhere observed. Urine drawn off by the catheter showed a quantity of albumen and a crowd of fibrous cylinders. Bleeding and opium resorted to, with apparently

good effects. The urine of the third day contained no trace of albumen. She rapidly recovered.

Dr. Hecker refers to another case, in which he examined the kidneys of a woman who had been convulsed during labour. They were very large, and presented an advanced stage of Bright's disease. The urine was albuminous.

Case 3. A woman, æt. 26, pregnant for the second time, was so unusually large that twins were suspected. Fourteen days before labour the legs began to swell, then the abdomen, then the eyelids. Urine drawn by catheter was repeatedly examined during the latter days of gestation. It threw down albumen copiously; and fibrous cylinders were also observed. She was delivered of twins. Ten hours after delivery the urine was alkaline, full of albumen, and the sediment contained a mass of fibrous cylinders. Thirty-six hours after delivery the urine was clear, neutral, and without a trace of albumen. *This patient had no convulsions.* She died three weeks after delivery, of peritonitis. The kidneys were pale, flabby, and bloodless, and easily separated from their capsules: their tissue exhibited no change under the microscope.

3. Professor Chiari has related the histories of three cases of pregnancy attending anomalous formation of the uterus:

I. *Pregnancy: uterus bilocular.*—A primipara, æt. 32, of small stature, weak conformation, with great distortion of the spine, was received into the Prague Lying-in Hospital. The fundus of the uterus reached the scrobiculus cordis; the head presented at the inlet of the pelvis. On examination, a fleshy septum was found dividing the vagina into two halves. By the finger, and by the speculum, the two halves of the os uteri were discovered. The right os was more easily permeable by the uterine sound than the left; and on the right side the head was more distinctly felt. Pregnancy in the right half was diagnosed. On account of the deformity of the pelvis it was determined to induce labour prematurely. The patient was in the 36th week. Scanzoni's method, by irritating the breasts, was first tried, without effect, during six days. Kiwisch's douche was tried: the first time, no result; the second trial was immediately followed by an eclamptic fit, and death in a few minutes. The child was withdrawn, living, by the Cæsarian section, but did not survive. The division of the uterus and vagina into two halves was found complete, down to the vestibulum. The right half was more developed: the placenta was adherent to the fundus of this cavity. The left uterine cavity had thick walls, not greatly differing in appearance from the right; the lining membrane was clothed with a villous, yellow layer, several lines in thickness. The length of this left cavity was nearly the same as that of the right, but its breadth was much less.

II. *Pregnancy: bilocular uterus.*—A primipara came into the hospital, having been delivered of a mature child, by breech-presentation, in the street. The cord had been torn off: the patient was bleeding profusely. The placenta was found adhering to the right side: in attempting to detach it the womb contracted irregularly. When removed, regular contraction followed, leaving the womb the shape of a ball. Peritonitis set in, and the patient died on the fourth day. Autopsy: septic endometritis, metrophlebitis and lymphangivitis plexus lumbalis, peritonäitis sinistra, laceration of the perinæum, œdema of the lungs of both sides, *uterus bilocularis.* The uterine walls were thick, and the cavity divided by a septum, two inches in length, into two halves. The placental-attachment was in the right cavity. The left half, also, was tolerably capacious: its long diameter, two inches; its transverse, nearly the same. (The condition of the lining membrane is not described.)

III. *Pregnancy: one-horned uterus.*—A woman was delivered in the hospital of a seven-months' child, which died of induration of the cellular tissue on the fifth day. The mother was affected with syphilis; she died on the eighth day of septic endometritis. The autopsy revealed a one-horned uterus: the left side entirely wanting. The left horn, the left tube, the left ovarium, and the left round ligament,

were equally wanting. The uterus was bent over to the right in the abdominal cavity.

Scanzoni relates a case of pregnancy in a rudimentary uterine horn, with probable passage of the ovum from the right ovarium to the left horn of the uterus. A woman in her fifth pregnancy was seized with pain in the left side, followed by repeated faintings. The general symptoms, and outward and internal examination, led to the belief of intra-uterine gestation, the escape of the fœtus into the abdominal cavity, and a profuse hæmorrhage. Death followed in half an hour. The autopsy presented a fœtus lying in the abdominal cavity. Below the fœtus a tumour the size of a fist was found connected with the fœtus and the surrounding intestines. On the right of this tumour was a second tumour, the size of a child's head. On removal it was found that the left tumour was the left rudimentary horn of the uterus; the right was the main mass of the organ. The two were connected together by a round string with a narrow canal. A tube and round ligament, leading to the ovary, terminated in each part. The left ovary presented several Graafian vesicles approaching maturity, several superficial scars, but no trace of a recently-burst follicle. In the right ovary there was discovered a three-cornered, callous scar; on the lower border of the inner circumference a bright-yellow corpus luteum. In this case, Scanzoni observes, the impregnated ovum must have passed over from the right ovary, across the right cavity of the uterus, and through the uniting canal, into the left rudimentary horn. This has never before been observed in a woman, although Bischoff has seen it in a bitch. The appearances present the most striking similarity to the preparation described by Rokitansky.

III. Labour.

1. *On the Causes of Indurations of the Placenta.* By Dr. Meckel. (Verhandl. der Ges. für Geb.—*Berlin,* 1853.)
2. *The Causes of the Death of the Child during Labour.* By Dr. C. Hecker. (*Op. cit.*)
3. *Reposition of the Prolapsed Umbilical Chord.* By Dr. C. Hecker. (*Id. op.*)
4. *Case in which the Umbilical Chord passed Eight times round the Neck.* By Dr. Crede. (*Id. op.*)
5. *On Premature Detachment of the Placenta.* By Dr. Crede. (*Id. op.*)
6. *Cæsarian Section.* By Kilian; reported by Dr. Sack. (Prag. Vierteljahrsch. 11 Jahrg. 1854. Erster Band.)
7. *Cæsarian Section.* By Prof. Faye. (Prag. Vierteljahrsch. 11 Jahrg. 1854. Zweiter Band.)
8. *On Paralysis during Gestation and Childhood.* By Dr. Fleetwood Churchill. ('Dubl. Quart. Journ.' May, 1854.)

1. Dr. Meckel has put forth some observations upon the different indurations of the placenta. These are sometimes accompanied by atrophy, sometimes, of the nature of tumours. The greater number of degenerations—which are described as induration, hepatization, fibroid, cysts of the placenta, and so forth—consist of various changes of parenchymatous or superficial extravasations of blood. The most frequent *cause* of extravasation of blood is congestion and inflammation of the maternal portion of the placenta. The inflammations of the mucous membrane of the gravid uterus spread to the placental portion, or, less frequently, to the decidua of the ovum. The latter gives rise to chronic induration, or acute softening, with small extravasations, and sometimes formation of pus.

2. Dr. C. Hecker has made an interesting contribution to our knowledge of the mode of death of the child during delivery.

The author sums up the evidence in proof that the placenta is a breathing-organ. He dwells upon the observations of Bayard, Casper, Ritgen, Cruvelhier, Litzmann,

and Krahmer, showing that in every case where the new-born child perishes from more or less complete occlusion of air, minute ecchymoses are seen under the pulmonary pleura. He also refers to the direct observations of Carus, Volkmann, Schneider, and Valentin, which go to show that whensoever the communication between mother and fœtus is cut off, symptoms of asphyxia, such as peculiar movements and efforts to respire, take place. He believes that these facts supply a new starting-point, whence useful researches into the cause of death of the fœtus during delivery may be extended. The interruption of the circulation between mother and fœtus may occur in three ways:—1. Through pressure upon the umbilical cord; 2. Through premature separation of the placenta from the uterus; 3. The normal attachments are preserved, but no blood proceeds from the maternal side to the placenta: this happens in the case of the mother's death.

Dr. Hecker illustrates by cases each of these modes of arrest of the circulation. He details six cases in which the child perished from pressure upon the cord. In all these, inspection of the body exhibited the characteristic ecchymoses of asphyxia on the pulmonary pleura, and general hyperæmia of the lungs. In one case, also, in which turning was resorted to, inspiration-movements were felt. (It is familiar to the practical accoucheur that movements, called convulsions, are commonly observed in the fœtus under the circumstances referred to by Dr Hecker. It is also well known that when they do occur the child's life is in imminent danger.)

He next relates two cases of death of the child through twisting of the cord round the neck. In these, also, asphyxial ecchymoses were observed. Then three cases are related of death from pressure upon the cord, arising from unfavourable positions of the child. Hyperæmia and ecchymoses were observed.

Three cases in which the child perished through premature detachment of the placenta are detailed. In these, identical post-mortem appearances were observed. Lastly, he relates the histories and results of the post-mortem examinations of three cases, in which the child perished in consequence of the death of the mother. The facts were of exactly the same order as in the previous cases. In one instance he remarks that the fœtus made unmistakable efforts to breathe, before its extraction by the forceps.

3. *Reposition of the Prolapsed Umbilical Cord.*—Dr. Hecker related a case of prolapsus of the cord; the head presented. He replaced the cord above the child's forehead, by introducing his hand. The child was born alive.

4. Dr. Credé related a case in which the cord was twisted eight times round the child's neck. The mother was affected with secondary syphilis; the child was apparently of the full time. The length of the cord was 57 inches. The death of the child was obviously caused by the strangulation, and had apparently taken place a few days before birth. The dissection exhibited no cause of death elsewhere, but the neck was so compressed, that it was not thicker than a thumb.

5. Dr. Credé has communicated the leading points of a work he has in the press on uterine hæmorrhages arising from premature detachment of the placenta or faulty attachment. In the case of considerable hæmorrhage during pregnancy, he avowed himself favourable to the use of the bladder or India-rubber bag filled with ice-water, recommended by Hüter, Gariel, Diday, and Braun. When labour has begun, he disapproves of the *tampon*. If the child lies in a longitudinal direction, then, in those cases where the placenta covers only a part of the os uteri, leaving another part free for the passage of the child, and even in some cases where the placental villi quite fill the os uteri, the descent of the head into the true pelvis pushes the detached portion of the placenta on one side, and compresses it against the walls of the uterus and the pelvis. Thus the child itself presses upon the bleeding spot. In those cases in which the blood flows fast at the commencement of labour, it completely stops as the child comes down. Credé observed this in five

cases—three times in head presentations, once with a nates presentation, and once with a cross presentation converted into a head presentation by turning. In four cases, the placenta projected over a part only of the os. In one case, its attachment was complete; in this instance it was pushed over to the left side, when it was tightly squeezed by the child's head. He rejected the method of Dr. Simpson, of detaching the placenta as early as possible, on account of the unavoidable destruction of the child.

6. Dr. Sack reports a case in which the Cæsarian section terminated favourably to mother and child. It was the seventh case of Kilian. A woman, æt. 32, a primipara, was admitted into the obstetric clinique at Bonn. The forceps were used without success, on account of narrowing of the brim. The incision divided the omentum; and on opening the uterus, the placenta was seen. The child was so tightly jammed by the head into the brim, that repeated strong pulls by the breech were necessary to extract it.

7. Professor Faye has related the history of a case in which he performed the Cæsarian section. The inlet of the pelvis was narrowed by two fibrous sub-peritoneal uterine tumours; these were pedicellated, and had fallen into the pelvic cavity. The issue was fatal to the mother. Professor Faye remarks that the Cæsarian section has been performed four times during the last ten years in Norway; that in all the cases the mothers died, and that in two the child lived.

8. Dr. Churchill has published a valuable essay on paralysis during gestation and childbed. After adverting to the absence of all notice of this affection in the great majority of obstetric writings, he cites the cases described by Ley, Scanzoni, Lever, Simpson, and Crosse, and introduces others communicated by Beatty, M'Clintock, Forrest, and Ireland, and some which occurred under his own observation. From an analysis of 34 cases, it appears that in 22 the attack occurred during pregnancy, and in 12, during or after labour. In 17 there was complete hemiplegia, and in 1, partial; in 4, paraplegia; in 6, facial paralysis; in 5, amaurosis; in 3, deafness. Four cases terminated fatally. It cannot be urged that congestion produced by the stress of labour, or pressure of the womb upon the pelvic nerves, is the cause. In two cases, the phenomenon followed anæmia from hæmorrhage. But the condition Dr. C. dwells upon, as offering the greatest prospect of solving the pathological difficulty, is the frequent coincidence of albuminuria. Dr. C. seems to incline to the opinion that some change in the blood is the cause. We may remark that the paper is a complete *résumé* of the subject.

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.

By W. B. Kesteven, F.R.C.S.,
Member of the Council of the Epidemiological Society.

I. Wounds, etc.

Effects of Combustion upon the Human Body.—A conflagration having taken place in a dense neighbourhood in Paris, involving the deaths of many persons, M. Tardieu, to whom the examination of the human remains was officially entrusted, has taken the opportunity of minutely observing and recording the effects produced upon the human frame by fatal burning.

The soft parts on the bodies examined were in various conditions: completely charred to cinder, partly carbonized, reduced to dried fibrinous shreds. The bones were dried and brittle, and in the instances of the long bones, fractures with obliquely splintered and charred ends were observed, differing distinctly from the characters of ordinary fractures. In the flat bones, which were thinned by the

heat, the fractures caused by the heat assumed the form of fissures confined to one surface, and not penetrating the substance of the bone. The intervertebral discs were contracted in their diameters. Teeth and cartilage seemed to resist the action of fire more than other hard parts. The soft parts exhibited great diminution of volume; this was more especially observed upon the viscera, which had been more or less protected from the immediate action of the fire. Some of these were mummified. The blood in the heart, aorta, and other large vessels, presented an extraordinary appearance, resembling wax, or fatty matter, of a most beautiful carmine colour. The cerebral substance was contracted to half its bulk, and in consistence resembled a half-dressed sweetbread.

To the details of the preceding, M. Tardieu has added the appearances discovered on the body of an infant that had lain several years behind a stove, and had become completely mummified. The effects of slow long-continued heat were much the same as the above described, with the absence of the active destructive action of fire seen in the carbonization of the external soft parts.—*Annales d'Hygiène, &c.*, April.

Death by Burning.—The body of a woman aged 64 years, was found burnt, at the door of an oven. The posture of the body was that of kneeling; the right hand rested with its knuckles at the edge; the head and the left arm extended, were within the oven; the right shoulder and upper extremity were outside, whereby the body had acquired an oblique position. Some suspicions having been excited as to the origin of this death, an official investigation was instituted, in order to ascertain if the body furnished sufficient indications to determine whether the burning had been accidental, intentional, or violent.

The head was so much burnt that the integuments of the forehead and left side of the face were completely charred. The neck showed traces of the action of fire, extending to the sternum. The integuments of the trunk were blackened posteriorly, as far as the sixth cervical vertebra. On the superior surface of the left breast was a distinct mark of the fire. Near this spot, vesication had taken place, the integument having been raised, the corium being of a dark-red colour. The left shoulder was burnt in one spot. The dorsal surface of the left wrist was deeply charred. The hand was firmly clenched by the flexor muscles. The skin of the arm and fore-arm was slightly charred, and partly mummified. The right upper extremity was also burnt about the shoulder; posteriorly, the integument of this limb, from the elbow to the axilla, was excoriated and slightly reddened, but presented no sign of the action of fire; on the knuckles of the ring and middle finger were three excoriations, but no trace of burning. An examination of the interior of the cranium exhibited very important injuries. The bones of the skull were blackened, or of a nut-brown colour. The membranes were thickened, and adherent to the bone. The surface of the cerebral substance was brittle; the usual moisture and fatty substance had disappeared, giving a resemblance to boiled brains. This change, however, did not extend to the central parts; the lateral ventricles and medulla oblongata appeared natural. The external surface of the cerebellum exhibited also this change in consistence; it was otherwise natural. The larger sinuses were empty, the other vessels moderately full. There was no sign of injury of any kind at the base of the skull or brain. The lungs were healthy. The heart was empty; the large vessels moderately full.

The commission appointed to investigate this case, after having inquired into all the circumstances connected therewith, gave it as their opinion that the burning had been inflicted during life, or instantly after death, as evidenced by the presence of vesication, but that it was impossible to determine from the position of the body, or the injuries found thereon, whether this had been accidental, intentional, or violent; at the same time they inclined to the first—i. e., accidental death—from the absence of all evidence that it had been caused by violence, and the improbability of its having been suicidal. The deceased had been subject to vertigo; it was possible that this might have occurred at the moment that she was engaged

about the oven's mouth, and that paralysis might have been instantly induced through the heat of the fire.—*Casper's Vierteljahrsschrift*, January, 1854.

Case in which it was doubtful whether Death had been occasioned by Drowning or by Violence.—About 2 P.M. of Feb. 3rd, the body of a man was found lying near the edge of a river. The person by whom it was observed, not knowing what it might be, endeavoured to drag it to the side with a pole armed with a hook; in this way he brought out from the water, first a leg, and then an arm, and at last the trunk itself to shore. On his return two hours afterwards, he found the body lying on its back several steps higher on the strand than where he had left it, having been rolled over by the water, the other leg hanging in the water. One of the persons who saw it at this time stated that blood flowed from the nose and mouth. The body was watched, but suffered to remain on the same spot, in the open air, until the evening of the next day—about thirty hours. It was then removed, and its external appearances noted. The body was identified as belonging to J. M——, of M——, who had left his home on the 3rd of January, and had not since been heard of. There were reasons for suspecting that he had committed suicide.

The sleeve of the coat was torn, as were also the knees of the trousers and stockings, and there was a hole, an inch and three-quarters long, in the upper leather of the boot. The body was much decomposed, and had apparently lain in the water from a fortnight to three weeks. On the vertex was a tumefaction to the extent of three or four square inches—how much elevated was not stated; on the forehead, a contused wound an inch and a half in length, and a quarter of an inch in width, having ragged, softened edges, from which blood flowed. The eyelids were swollen, the integuments had disappeared from the nose, the tongue was between the teeth, the abdomen was distended. Both knees presented abrasions, adherent pebbles, &c.

On the 5th of February, the body was opened and examined internally. The stomach was found to contain undigested potato, &c., but neither sand nor water. The diaphragm was not convex towards the abdominal cavity. The lungs were collapsed, leaving the heart exposed; of a bluish colour, more or less adherent to the costal pleuræ, and when incised did not appear congested with blood. The heart and large vessels presented nothing unusual. The trachea and larynx were free from water or sand. The cranium was denuded of scalp and pericranium on the forehead, to the extent of more than an inch and a half. The scalp was, for an extent of four or five inches, loaded with dark blood, and œdematous. The skull bones were uninjured; nothing abnormal was perceptible between these and the dura mater. At that portion of the brain corresponding to the external sanguineous infiltration of the scalp, congestion of the cerebral vessels was observed—in other parts this was wanting; the sinuses were empty. The brain was healthy, so far as could be judged.

The inference drawn from the appearances thus described was, that the deceased had met his death from violence applied to the head, and that the body had been placed in the water after death;—that drowning was not the cause of death.

Dr. SIMEONS, of Mentz, combats this conclusion. He remarks, that many of the usual signs of drowning having been absent may be accounted for by the length of time that the body had lain in the water, and from the length of the period during which the body had afterwards lain exposed to the air. The aspect of the wound of the scalp, and the discharge of blood thence, served to indicate that it had not been inflicted during life, or immediately on its cessation. It was scarcely probable that a wound so inflicted upon a body which had lain nearly three weeks in the water, would bleed afresh at the end of that period; neither was it probable that so firmly adherent a membrane as the pericranium would have been so extensively separated as was found in this case. The existence of such lesion was far more readily explicable by supposing it to have been accidentally occasioned by post-mortem violence, in dragging the body out of the water on to the bank. The

œdema, &c., of the vertex, Dr. Simeons held to be reasonably explained as having arisen from the head of the deceased having come into contact with the ground, or some hard substance, at the time that he plunged into the water, supposing suicidal drowning to have been the cause of death. There was an absence of all appearances within the cranium indicating such an injury to the skull as would have proved fatal. The consequence of so long a submersion is, that blood would flow freely from the integuments when cut or lacerated. The deceased had dined just before he left his master's house, and on examination of the body the undigested food was found in his stomach, proving that death had taken place within a very short period from his having left the house. Besides these considerations, other circumstances afforded support to the opinion that suicide had been committed.—*Casper's Vierteljahrsschrift*, January.

The Medico-Legal Relations of Injuries of Doubtful Origin.—Cases are of frequent occurrence which, from their character, it is difficult to say whether they have originated in accident, intention, or violence. The following instances are related by Herr Hqdann, to illustrate the difficulties in question.

1. A cooper had received two wounds on the front of the fore-arm, extending to the wrist, the one dividing the radial, the other the ulnar artery. The loss of blood was considerable, and the patient was exhausted. His own account of the accident was, that he had, with two knives in his hand, slipped from a narrow plank into the water, and that he had in this way caused the two wounds. This history was confirmed by eye-witnesses. If these had not attested the accuracy of his statements, and rendered him aid, this man might have died in the water, and it would then have been a difficult question to have determined how he had come by his death.

2. The body of a woman, about forty or fifty years of age, was found in the Oder, far advanced in decomposition. In the pockets of the dress were several heavy stones, and round her neck was the rope of a clock, from five to six times tightly twisted, the end passed behind the ear, where it was tied in a firm knot. The larynx was compressed thereby, but not broken. No other injuries were observable. So far as the process of decomposition permitted observation, there was an absence of congestion of the brain, and organs of the chest.

Had the deceased died from hanging or strangling? How, under either circumstances, did her body come into the water? It transpired subsequently that she had left her home in a depressed state of mind, having written a letter in which she declared her intention to put an end to her existence. It seemed most probable that she had, in the first place, unsuccessfully attempted suicide by hanging; and that failing, she had completed self-destruction by drowning.

3. The body of a man was found in some water, with the legs firmly tied together; around the right wrist was another cord, the free end of which passed round the back, and was fastened on the front of the body to a loop at the other end of the cord, by an unusual knot. There was an entire absence of all other violence, or of robbery having been committed. The case was doubtless one of suicide.

4. A young girl, through disappointed love, determined on suicide. For this purpose, she inflicted such repeated blows on her head with an axe, that the integuments were reduced to a pulp, the periosteum was torn off the bone, and the bone itself depressed. She recovered from these injuries, but subsequently died insane. Had she been found dead, with the axe beside her, it would have been concluded that she had been murdered.—*Casper's Vierteljahrsschrift*, January.

II. Toxicology.

Death from Chloroform during Labour.—A lady, twenty-five years of age, in full health, was in her second accouchement. Labour had progressed slowly for above thirty hours, when she expressed a wish for the inhalation of chloroform, which she

had used in her previous labour. Her medical attendant, Dr. Freeland, advised against its use, and after waiting a few hours, bled her to fifteen or twenty ounces. The case having made but little progress, forty drops of tincture of opium were given, and the patient obtained some rest. When she awoke, she again importuned for chloroform, on account of pain in the abdomen and loins. Her pulse was strong and full, not exceeding 100; the tongue moist and clean; uterine action tardy; os uteri yielding; head advanced; pelvis capacious; no unpleasant symptom. Ergot of rye was then given. The patient insisting on having chloroform, Dr. Smith was sent for, who brought a small bottle containing not more than two ounces. This he placed on a table, within sight of the patient. While Dr. Smith was receiving Dr Freeland's account of the case, she obtained possession of the bottle of chloroform, and refused to give it back, inhaling it from time to time. When remonstrated with by the physicians on the peril she incurred, she answered that her pains were "quite comfortable." In this condition she remained for twelve hours. Upon a careful examination, no material change in arterial action or nervous power was discovered, but very clearly, as the physicians thought, a promising change in the rigidity of the organs; and the chloroform being gone, they felt confident there would soon be increased uterine action, and a "triumphant" finishing up of the case. Absence, however, of all pain, a cold sweat, cold extremities, oppressed and whizzing respiration, receding pulse, and "vacant glare," pointed to a sudden and fatal termination. Delivery was immediately effected; stimulants, &c. were employed; but the patient died in about ten minutes afterwards.

This case is quoted from the report thereof in the 'Buffalo Medical Journal,' December, 1853, by Dr. De Wolf, who had been called in consultation at the last; and is given in Dr. Henry Littlejohn's report on toxicology, in the 'Association Journal,' May 26th.

We cannot refrain from expressing some surprise that a patient, under the above circumstances, should be allowed by her medical attendants to have possession of a two-ounce bottle of chloroform, and repeatedly inhale its contents during twelve hours. The exhaustion of a previous lingering and painful labour, of thirty or forty hours' duration; the loss of from fifteen to twenty ounces of blood; and the administration of forty drops of laudanum to procure sleep—as they were not at any time the most favourable preparations for the inhalation of chloroform, so they formed, in this case, the more cogent reasons why it should have been given with the very greatest care, instead of being, without precaution, allowed to be loosely sniffed during twelve long hours. No apology can be grounded on the statement that the patient surreptitiously obtained the bottle, and determinedly retained possession thereof. The duty of a medical attendant is often more imperative in the direction of protecting patients from themselves, and the consequences of their own perversities, than in relieving them from the effects of pathological or extraneous influences. Firmness of purpose in such a case should go beyond verbal remonstrance, if the attendant would avoid painful after-reflections, and some remorse.

Another death from chloroform, which occurred in Paris, is recorded in the 'Lancet,' April 9th. In the weekly journals for May, two more cases are detailed, one having taken place in St. George's Hospital (the first that had happened there), and the other at the Lock Hospital. A death from the same cause, which took place in Sheffield, is related in the 'Association Journal,' April 7th.

Attempted Murder with Nitric Acid.—The dead body of a woman was found, presenting abundant traces of the action of nitric acid in the mouth, on the face and upper part of the body and garments; the throat, &c. also exhibited marks of violence, such as abrasions, impressions of fingers, &c. Similar stains on the clothes of a man with whom she had cohabited, and with whom she had been seen a short time previously, were proved by chemical analysis to have been caused by nitric acid. On examination of the body, it was found that the nitric acid had not reached the stomach. It was suggested that this was not necessary to the fatal

result, as death may have been caused from suffocation induced by the irritating effect of the acid upon the glottis. The suggestion is, however, as M. Chevallier observes, merely theoretical. The congested state of the brain and lungs, together with the injuries on the neck, rendered it more probable that death had occurred from strangulation, and that the fatal consequence may have been favoured by the deceased having been intoxicated. We may observe, however, that both Dr. Christison and Dr. Taylor state the possibility of death being caused by the action of a mineral acid as here suggested; and although regarded by M. Chevallier as a mere hypothesis, the fact is cited by Dr. Taylor as having been met with by Mr. Quain in 1836, and by the late Dr. A. T. Thomson in 1837.*

This case occasioned the questions—Whether the stains of nitric acid applied to the living body differ from those it produces after death? How long these would remain? and Whether other acids would cause similar effects? M. Chevallier observes, that the effects of nitric acid applied during life vary with the degree of its concentration and the duration of its application. Strong acid will produce a greyish eschar, more or less deep, surrounded with a more or less intense yellow ring. Simple epidermic stains are first of a canary colour, becoming, after some hours, of a deep orange hue, remaining until the epidermis wears off. Applied to the dead body, nitric acid produces stains of a sulphur or yellowish-green colour, which, in about ten or twelve hours afterwards, becomes surrounded with a more or less extensive grey tint, evidently from imbibition of a portion of the acid; towards the third day, this greyish stain presents, at its limits, a pale violet hue. These characters will last for about seven days.

Experiments performed by M. Chevallier, with sulphuric, hydrochloric, and oxalic acids, show that each of these presents particular characters, but all differing from those of nitric acid.—*Journal de Chimie Médicale*, February.

Death from Sulphuric Acid.—Mr. James Heywood, a chemist in Sheffield, met his death by falling forwards on the ground where a large quantity of sulphuric acid had been spilt from a carboy. The fatal result, which took place five hours afterwards, appears to have been caused by the irritating action of the fumes of the acid, or by the actual application of some of the acid to the glottis.—*Morning Herald*, March 10th.

Poisoned Confectionery.—The 'Lancet' Analytical Sanitary Commission gives the following summary of 101 analyses of coloured confectionery, of the sugar-plum and sweetmeat order. The colours were given by various substances—e. g.:

Yellows—In 59 samples, the colour was given by varieties of chromate of lead; and in 11, by gamboge.

Reds—In 60, this colour was derived from cochineal; in 12, from red lead; in 6, from bisulphuret of mercury.

Browns—8 were coloured with brown ferruginous earths.

Purples—2 were coloured with a mixture of Prussian blue and (probably) cochineal.

Blues—1 was coloured with indigo; 21 with Prussian or Antwerp blue; 15 with German or artificial ultramarine, a sulphuret of sodium and aluminium.

Greens—10 samples were coloured with varieties of Brunswick green, a mixture of Prussian blue and chromate of lead; 1 was coloured with carbonate of copper; 9 with Scheele's green, or arsenite of copper.

Several of these colouring ingredients were variously combined in some samples. In all the cake ornaments, the colours used were painted on with carbonate of lead.

The majority of the above colouring matters are among virulent or deadly poisons. The quantity is not in all cases so small as supposed, as proved by

* See Taylor on Poisons, p. 222; Medical Jurisprudence, 4th edition, p. 43; Christison on Poisons, p. 157.

instances frequently occurring of disease, and even death, following upon their ingestion; moreover, it must be borne in mind, that the effects of lead, mercury, and arsenic, are cumulative.

In France, Belgium, and Switzerland, the employment of poisonous colouring-matters in articles of confectionery is penal; and the venders are held responsible for all accidents which may be occasioned thereby.—*Lancet*, May 26th.

Poisonous Contamination of Alimentary Matters.—In the 'Medical Times and Gazette' of April 15th, Dr. ALFRED TAYLOR has related the circumstances under which he detected the accidental contamination of the bread placed on his table, with Scheele's green.

In the 'Journal de Chimie Médicale,' February, there is also an account of the ill effects of fly poison, which had been mixed in cheese in order to prevent the development of maggots! and by which several persons were seriously affected.

Poisoning from Ænanthe crocata.—A woman, aged 24 years, for the relief of erythema, on going to bed, drank a strong decoction, under the name of "herb tea." She was very soon after seized with faintness, purging, and vomiting, followed by convulsions, and died within an hour after having taken the poison, which was ascertained by Dr. NICOL, of Swansea, to have been hemlock water dropwort (*Ænanthe crocata*), and the wild celery (*Opium graveolens*).—*Association Journal*, March 10th.

Poisoning with Hyoscyamus niger.—Dr. SCHLIZZI, of Aigues-Mortes, relates that several persons accidentally partook of a considerable quantity of the roots of this plant, which they had mistaken as edible. They were instantly seized with the symptoms of poisoning, varying in severity according to the quantities severally eaten. Those who had taken a moderate dose presented all the characters of mania; those who had taken rather more became apoplectic, and as this condition passed off, exhibited maniacal excitement; lastly, those who had partaken most largely of the roots, suffered also from tetanic symptoms similar to the effects of strychnia. The quantities taken are not stated.—*Journal de Chimie Médicale*, March.

Chronic Poisoning with Ergot of Rye.—A girl was admitted into the Hôpital Cochin, under M. MAISONNEUVE, who had lost the use of her fingers for a month previously. One of the last phalanges in both hands was of a deep black colour; the lips of the others were purple and cold. The fingers themselves were stiff, cold, shrivelled up, and painful to the touch; while the hands were covered here and there with red spots, like erysipelas. The pulse was perceptible at the wrist. The feet were swollen, but presented no sign of approaching gangrene. She stated that the limbs were never the seat of any spontaneous pain, and that she had felt no giddiness at the beginning of her illness. Previous to the appearance of the gangrene she had been regular, but since then she had never menstruated. For three months prior to her illness, she had partaken of no injurious food.—Quoted from 'Gazette des Hôpitaux,' February 11, in Dr. Littlejohn's 'Report on Toxicology' in the *Association Journal*, May 26th.

MEDICAL INTELLIGENCE.

[THE following suggestive letter from DR. BENEKE, on a mode of examining into the qualitative alterations of the blood and urine in zymotic diseases, has been communicated to the Epidemiological Society, and we have been requested by the Honorary Secretaries to publish it.—EDITOR.]

MY DEAR DR. SIEVEKING,—I should feel very much obliged if you would let me know whether any steps have been taken by the Epidemiological Society, to come to an accurate knowledge of the obscure chemical changes of the constituent parts of the blood, which take place in patients suffering from zymotic disease.

From some observations which I have made with regard to this point, I am convinced that co-operation of many members of the profession on the same path would throw much light upon the nature of these diseases; and more especially, I may say, that the accurate examination of the qualitative changes of certain substances, which are mixed with the blood or the urine of patients suffering from zymotic disease, would be of the greatest importance.

Nothing has been done hitherto to solve the question, whether the qualitative changes of substances in the zymotic patient take place in the same way or in a different one, as in healthy persons, or patients not suffering from zymotic disease. We know that there are substances in the healthy economy which act by a so-called catalytic power, like a ferment on other substances; we know that, by such fermentation, one kind of the so-called proteïn-compounds is changed into another one, that amylon in this way is changed into grape-sugar, grape-sugar into lactic acid, &c., &c. Are not these fermentations (to use a short expression), important as they are for the healthy economy, are they not altered in disease? May not an alteration of them be of the same importance as many alterations of the quantity of certain constituent parts of the body in other diseases? Is there not much reason to suppose that, especially in zymotic diseases, they are of a paramount importance? Is it not very likely, that those fermenting substances can exceed the normal quantity, as well as remain beyond the normal standard?—that certain fermenting substances are produced in disease, which do not appear in the healthy state?

From my own observations, I only know, that the urine of different times of the day, as well as of different patients, requires a very different time for decomposition; it appears to me, that persons suffering from zymotic disease void a urine which is much earlier decomposed than urine of healthy persons or individuals suffering from any other complaint, supposing the acidity of the different specimens to be the same. Again, urine of different times of the day, as well as of different patients, when mixed with grape-sugar, shows quite a different action on the sugar—viz., on its decomposition into vegetable acid and water. Hence I only conclude, that there *exist* alterations, whose accurate investigation must lead to most important conclusions on the nature of the diseases themselves.

But trifling as they are, I should have been far from laying any weight upon these observations, if they were not supported by the most excellent inquiries of the well-known physiologist, C. Schmidt, of Dorpat, regarding cholera, and likewise by Professor Frerich's investigations on uræmic patients.

In page 68 of his excellent work on 'Cholera,' Schmidt adduces the conclusions from his experiments in the following words:

"The normal blood of a healthy person, and more especially the blood-globules —not the serum sanguinis—examined at the time when cholera was prevailing amongst the inhabitants, contain a substance, by the self-decomposition of which a ferment for sugar originates, and another substance, by the self-decomposition of

which a ferment for urea is produced. In a cholera-patient, however, who was attacked four hours before, the former ferment was not to be detected—it seemed to have been transformed into the latter; and in another cholera-patient, who suffered from the disease since forty-eight hours, sugar, mixed with the blood, was in full decomposition on the third day, and urea on the fourth day; sugar gave here three times the quantity, and urea ten times the quantity of decomposed matter, as they did when mixed with the blood of the healthy individual—namely, carbonic acid and ammonia. Moreover, in this patient, a formation of emulsin (the ferment for amygdalin), which could not be detected in the healthy individual and in the first cholera-patient, took place a short time after the formation of the sugar- and the urea-ferment."

Professor Frerichs, on the other hand (on page 167 of his elaborate work on 'Bright's Disease,' 1851), came to the conclusion, that the uræmic intoxication is not caused by the admixture of urea, nor of any other constituent part of the urine to the blood, but only by the generation of a ferment, which acts on the decomposition of the urea into carbonate of ammonia.

These observations, striking as they are, appear to be of the greatest importance for the whole doctrine of the zymotic diseases. They clearly show that, under certain circumstances, fermenting substances are produced in the blood, which in such a quantity or quality are not to be detected in the healthy state. It is very likely that similar circumstances, as in the blood, are to be detected in the urine, and it really seems worth while to take this subject into the most careful examination.

I think, therefore, it would not be improper to propose the following question: When certain quantities of blood or urine are mixed with certain quantities of grape- or milk-sugar, or of urea, or of amygdalin, what difference exists with regard to the fermentation or decomposition of these substances, when blood or urine of healthy persons is employed, and when blood or urine of patients suffering from zymotic disease, is used?

Regarding the urine, I have made my first experiments with five equal portions of it at the same time, in order to know whether boiling of the urine, or the elimination of the extractive matters, might cause an alteration of the decomposition. The first specimen was only exposed to the influence of the air of a certain temperature; the second was boiled, and then left to the influence of the air; the third was mixed with a certain quantity (0·5 grammes) of milk-sugar, and then left to the influence of the air; the fourth was first boiled, then mixed with the same quantity of sugar as the third, and then exposed to the air; and in the fifth specimen, the extractive matters were thrown down by acetate of lead, then the urine was filtered, mixed with sugar, and exposed to the air. On each of the following days, each specimen was examined with regard to the reaction and the absence or presence of sugar by Trommer's test.

With regard to the blood, it would be necessary to examine the unaltered blood as well as the blood-corpuscles, the fibrine and the serum sanguinis separately; and these examinations should be exhibited on a great many healthy persons, as well as on a great many persons suffering from different, and especially from zymotic, diseases.

These short remarks, my dear Dr. Sieveking, I take the liberty of communicating to you, and you will be kind enough to tell me whether any inquiries in this direction have been made already by the Epidemiological Society. If not, I leave it to you whether you think it proper to lay the question before the members of the society; I should be very much pleased if their scientific co-operation were to contribute to an early investigation of a subject, which seems to me of the greatest importance physiologically, as well as therapeutically.

With the kindest regards,

I remain, my dear Dr. Sieveking, yours very sincerely,

Dr. F. W. Beneke.

Oldenburg, January 30, 1854.

The Elections at St. Bartholomew's.

We cannot pass over without a word of remark the late elections of Drs. Baly and Kirkes, at St. Bartholomew's Hospital. They have an interest beyond the walls of that institution, for a great principle was involved—a principle which it is the peculiar honour of this age to have recognised and enforced. It would, indeed, have been a retrograde step, most deeply to be lamented, if, at this time, when our rulers are throwing down the exclusive barriers which have hitherto kept the body of the people from the posts which industry and talent may justly claim, the governors of St. Bartholomew's Hospital had reverted to the custom of allowing the claims of interest and relationship to override all others. Such an abuse of public duty might, indeed, have been attended with one good result: it might have called forth such an expression of public opinion as would have led to some modification of the present absurd practice, which assumes that an annual subscriber to a hospital is the best judge of the merits of the men who carry out the purposes of the charity.

The interest of this election centred only in the choice of the assistant-physicians, as there was, fortunately, no opposition to the election of the most competent man to be the new assistant-surgeon.

For the post of assistant-physician, a gentleman who has an European reputation, who has been selected for honourable employment by the government of the country, and who has been for many years a teacher in the medical school of St. Bartholomew's Hospital, presented himself.

It could scarcely be credited that there was for one moment a doubt of Dr. Baly's success. Yet such was the case when it became known that a young physician had come forward, whose claims were based neither on services to science nor on labours in the medical school, but simply and entirely on the ties of blood with the senior physician of the hospital.

When, however, it became evident to the advocates of nepotism, that Dr. Baly's claims could not be successfully set aside, a movement was made which had very nearly the effect of bringing victory out of defeat. A new vacancy was created, into which it was hoped the unsuccessful candidate for the first vacancy might quietly pass. We can now scarcely regret this attempt, since it failed in its design, and has had the effect of bringing Dr. Kirkes into a new sphere of activity and usefulness; but we must say that the authorities who could thus coolly consult the interests of a single man, without the least consideration of the sick persons, to afford the best aid to whom is the object of the hospital, have been guilty of a most grave dereliction of public duty.

How this attempt was defeated we need scarcely say. The same arguments which were urged in favour of Dr. Baly against Dr. John Hue, were equally operative in favour of Dr. Kirkes. By the most strenuous exertions, the real question at issue, merit or nepotism, was urged upon the governors, and by a narrow majority, the man who deserved the post was chosen to fill it. But the large number of governors who voted in favour of Dr. John Hue, who, whatever may be his talents and future services, has at present no claim to be the assistant-physician of a great hospital, must satisfy every one that, in spite of the present triumph, the claims of merit are not secure in any future election.

We trust, then, that those who have fought this battle will remember, that a victory not followed up is a victory thrown away. Now is the time, long looked forward to, long striven for, the time to introduce some better system into the elections at our great hospitals. The present plan is rotten throughout. Improper in its principle, it cannot be right but by accident, in its practice. There is no reason why, because a man wishes to aid the poor with his money, by subscribing to a hospital, that to him should be intrusted a duty he cannot properly exercise, that of choosing those who are to carry out the objects of the hospital. If any man subscribes to a hospital, in order merely to gain a vote at an election, it is money given for a base purpose, and not in that pure spirit of charity in which

alone the offering should be made. The authorities of a charity like St. Bartholomew's should scorn to accept the money which is given merely to serve a selfish purpose: let them boldly take away the privilege of election from their subscribers, and vest it in some competent and responsible body. Then they will do away with the reproach, that any man who chooses to subscribe his pittance to St. Bartholomew's Hospital should have the power to thwart the noble purpose of its royal founder, who did not intend it to be the pleasant *appanage* of family interest, but to be the scene of a grand work of beneficence, from which men could offer up their earnest labours in the cause of suffering humanity, to Him in whose honour the ancient portals of the hospitals were, in old time, raised.

Nurses for the Poor.

We are happy to observe that the Epidemiological Society has appointed a committee to consider an important subject, which has been very strenuously urged on the attention of the profession by Dr. Sieveking—viz., the desirability of providing nurses for the poor in their own homes. Those who know the homes of the London poor, and who see how hardly sickness presses on a family, and how invaluable the aid of a nurse would be when a mother of a family is on a bed of sickness, or when the father is for the time incapacitated for work, and when his wife has been worn out with the treble duty of attending to her sick husband, and to her family, and of providing, it may be, the means of living, will at once admit that few philanthropic problems are more necessary to be solved than the one which the Epidemiological Society have undertaken to consider. The plan proposed by Dr. Sieveking is to instruct the women in the workhouses, who at present have little or nothing to do, in the duties of nursing, and to send them out gratuitously, or at a small charge, to such poor persons in the parish as may want such assistance. We have not space at present to discuss this proposal, but it appears a very feasible one, and it has the double advantage of giving help where help is needed, and of supplying useful occupation where occupation is wanted. We trust that the committee may receive the support and encouragement which they deserve.

Sir John Forbes' Medical Bill.

At the moment when the movement for medical reform seemed almost ended for the time by the dissensions of its promoters, a new phase has been given to the subject by the Bill prepared by Sir John Forbes. We should say, did not experience teach us to be chary of our hope, that it is probable this Bill may lead to a settlement of the question. It is a judicious adjustment of the claims of the profession and of the corporations, and of the requirements of the public.

We shall not enter into the details of the Bill: this has already been sufficiently done in the weekly journals; and we need only say that it contemplates the formation of a central council, under whose control will be put all the details of medical education, while at the close of their students' career, and after undergoing certain preliminary examinations, candidates for the profession will be obliged to join the Colleges of Physicians or Surgeons. The Society of Apothecaries will suffer political extinction. In choosing the members of the council, we hope the most liberal course will be taken, and that the general practitioners will be called upon to furnish their proper proportion, either by the plan submitted in the alternative scheme of the third clause, or in some similar way. The Bill also would cause the enforcement of a complete register of medical men, and would also render illegal the practice of medicine, or the assumption of medical titles, by unqualified persons.

Even this brief sentence will show that the Bill will effect a radical change in medical education, and in the position of medical men with regard to the public.

And yet, with this vast change, it still keeps up, and endows, indeed, with a new life, those old corporations which the public generally believe to be identical with the medical profession itself. The time-honoured names remain to us; and the Colleges of Physicians and Surgeons, which, in spite of all shortcomings, are the representatives of the profession, and are indelibly connected with the history of British medicine, will still remain as household words among the people of this country.

It has been reported, and we hope with truth, that the Corporations are disposed to look favourably on this Bill. They could do no wiser thing. Their privileges are left intact: they keep everything they ought to keep, everything they could hope to keep, and much more, one should think, than they expected to keep. Their course is clear: if they wish to continue to exist, they must conform in time to the demand for Reform; a demand which may be at present baffled, but must ere long be satisfied.

As to the profession at large, we would most strongly urge them to support this Bill. It will give us the greater part of what we want, and all we could hope to gain. It gives us three benefits—viz., improved education, medical registration, and protection from quackery, as far as it can be done. For such tangible boons as these, we may well submit to sink minor differences of opinion, and even to postpone, *sine die*, the profound organic changes some have contemplated. It might be possible to devise a more sweeping and democratic scheme, but it would be hopeless to think of carrying it into law.

We have no doubt that, if we were to exert ourselves, it would be possible to get this Bill passed even during the present session. The Provincial Association have given up their own Bill; they have here a much better one. If the leaders of that body wish us to believe that they are sincerely desirous to benefit the profession, they have now their opportunity. The Association has a potent organization, and represents a large portion of the most educated medical men. If they could take the suffrages of their constituents, we cannot doubt what the result would be.

If the Corporations whose interests are so carefully cared for, and the Association whose objects are so completely attained, were to signify their support of this Bill to the Home Secretary, we can scarcely doubt that the government would be only too glad to pass this measure, and thus to end the vexatious question of medical reform.

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APPENDIX.

ANNUAL REPORT OF CASES ADMITTED INTO THE MEDICAL WARDS OF ST. GEORGE'S HOSPITAL,

DURING THE YEAR ENDING DEC. 31ST, 1853.

BY DR. BARCLAY,

MEDICAL REGISTRAR OF THE HOSPITAL.

During the past three years a uniform system of registration of medical cases, and classification of diseases, has been adopted at St. George's Hospital, with the view of ascertaining any great, leading facts with relation to the prevalence of disease and the ratio of mortality, which from time to time the statistics of an hospital may serve to elucidate; and, in conformity with this plan, an Annual Report has been presented to the Governors, embodying the results of this registration. By mutual co-operation unquestionably much valuable information might be accumulated from the various hospitals, especially those connected with the metropolis, where talent and industry are never wanting to carry out any object that may tend to the advancement of science; and by the suggestions of others engaged in similar pursuits, the purposes to which registration may be made subservient would be more fully developed than has been attempted in the accompanying table.

It is here presented simply in the form in which it was submitted to the governors of the hospital, as the 'Annual Report of Medical Cases for the year 1853,' with the addition of two columns containing the total admissions, and the percentage of mortality for the three years. The cases, when first admitted into the hospital, are entered in the ordinary register of patients, and after death or discharge each case is transferred to a classified index of diseases, which forms the basis of the annual summary. By this arrangement a considerable amount of information regarding any particular disease is at once available: the age, sex, and occupation of the patient, the duration of the disease prior to admission, the period during which the case remained under treatment, and its results, as well as the diseases with which it was complicated, can readily be ascertained with reference to any of those enumerated in the index of diseases.

In the accompanying Report, whenever these complications have been deemed of sufficient importance, the particulars regarding them have been introduced in the notes, especially with reference to the fatal cases. But though the materials for a more complete enumeration exist, it was found impossible to tabulate them in such a manner as to bring the statement within the moderate compass of a yearly abstract.

Hitherto it has been limited chiefly to the development of three important particulars:—the actual number of examples of each disease admitted during the year, the ratio of mortality, and the proportion of cases complicated with other

diseases; and in order to do this with any degree of accuracy, the report for the year must always be delayed for two or three months after its close, to allow time for the termination of the cases. Its exact character is a report of diseases treated in the hospital during the year, not of patients admitted; consequently, the sum of the numbers given does not represent the actual number of persons who were the subjects of these diseases. For while each patient applies for admission on account of the urgency of some particular symptom, and might thus be classified solely according to the most prominent form of disease under which he is labouring, true statistics of the relative mortality of each disease could not possibly be thus attained. For example: a patient is admitted with dropsy;—this is in itself a disease altogether independent of the organic lesions with which it is associated, coming on sometimes quite irrespective of them, or absent in spite of their existence. This patient may have disease of the heart, or disease of the kidneys, or both, and he may have in addition a distinct attack of bronchitis, without which he never would have had dropsy; another patient applies for disease of the heart without dropsy; a third for bronchitis alone; a fourth for bronchitis with disease of the heart, and so on; and it is quite manifest that in seeking for statistics of these several diseases, a patient with disease of the heart must be classed under that head, whether he have dropsy, or bronchitis, or albuminuria, or not; and one with bronchitis must be classed under that head, whether he have diseased heart or kidneys, or dropsy, or none of these diseases: in short, that the same case must be entered under each head, and appear under each as one of the complicated cases.

In pursuance of this plan, the first column gives the total number of cases under each head, entered in the patients' register for 1853 as having suffered from that particular disease, whether primarily or secondarily, whether admitted labouring under it, or having been attacked by it while under treatment for some other malady; the second column gives the total deaths, and where the disease under which they are enumerated seemed to have nothing to do with the fatal result, the fact is recorded in the notes; the third gives the ratio of mortality calculated from the previous numbers; the fourth and fifth columns give respectively the number of cases in which some other disease was present in addition to that under which they are classed, and the deaths among these complicated cases; the sixth and seventh, as already stated, give a general summary of the three years.

Further information may be obtained by a comparison of these several columns with each other: by subtracting the fourth and fifth from the first and second, we may ascertain in what proportion any given disease is liable to prove fatal, in a healthy individual, uncomplicated by any other malady; while we also learn, in a general manner, the proportion of complications in different diseases. Again, the sixth and seventh columns, as compared with the first and third, show, by an accumulation of three years' observations, the increase or diminution of each form of disease during the past year, and its more or less favourable rate of mortality.

A few words must be said of the mode of classification which has been adopted. It is based upon that of the Registrar-General, but does not entirely coincide with it; and though there is manifestly a disadvantage attendant on want of uniformity in tabular arrangements, as it interferes with a ready reference for the purposes of comparison, yet the alterations seemed imperatively called for, and were not made without due consideration; they were finally adopted, with the concurrence and sanction of all the physicians of the hospital, from whom most of the suggestions originally proceeded.

Scarcely any two authors agree in their manner of classifying diseases; and therefore, without giving the preference to one over another, where so many have equal claims, it was judged best to assimilate that which was prepared for the hospital as much as possible to the weekly returns of mortality. But it must be remembered, that while the one is a statement of cases of disease, the other is a report of causes of death, and many of the diseases enumerated in the former find no place at all in the latter. But, in addition to this, the classification of the

Registrar-General is not altogether free from objection. Take, for example, diarrhœa, as classed among zymotic diseases. It is true that, at certain seasons of the year, some of the examples of diarrhœa might be so classed; but of the cases admitted throughout the year how few are really examples of a zymotic disease! In many instances it is a concomitant of phthisis; in some, of albuminuria; and in not a few, is a consequence simply of injurious aliment. It therefore seems more natural to place it among the diseases of the intestinal canal, to which class dysentery has also been removed, as it is commonly seen only in its chronic form.

Indeed, there are serious difficulties in the way of the adoption of the whole class of zymotic diseases, and it has been thought better to break down the first three divisions of the Registrar-General into a number of separate heads, which will be found to succeed each other in distinct groups. Febrile diseases stand at the commencement, amongst which acute rheumatism and gout would naturally find a place; and, simply as matter of convenience, the chronic forms of the same diseases are placed in juxtaposition. Next follow diseases produced by adventitious causes, such as poisons, &c.; and then those which have been classed as diseases of uncertain and variable seat,—dropsy, hæmorrhage, anæmia, &c. These are followed by depraved constitutional states,—scrofula, tubercles, and morbid growths; and next in order stand the quasi-nervous diseases, followed by the true diseases of brain, spinal cord, and nerves; after which each set of organs is taken in succession.

Hydrocephalus has not been separated from inflammation of the brain; hooping-cough and croup have been placed under diseases of the respiratory organs; erysipelas has been regarded as an eruptive fever; chlorosis has been recognised as distinct from anæmia; and amenorrhœa without anæmia has been classed as a disease of the uterus itself. Ascites has been removed from diseases of the organs of digestion and placed under dropsy, anasarca being used as the generic term for general dropsy, even when ascites was present, if the case was characterized by a tendency to general infiltration of the areolar tissue.

Such are a few of the more important changes in arrangement which necessity or convenience has dictated; but there are yet many difficulties in the way of a correct enumeration of cases, of which the whole class of hæmorrhages afford numerous examples. Reference has been already made to diseases of the intestinal canal, and perhaps this is one of the most imperfect. Ulceration occurs in the course of fever and of phthisis very frequently, and is probably always present in chronic dysentery; yet, in the absence of post-mortem evidence, its existence can only be inferred, and, consequently, the cases classed under this head have been limited to those arising from some other cause. Similarly, diarrhœa occurs in the course of various diseases, but it constantly happens in hospital practice that the primary disease is in a quiescent state, that the patient is only conscious of suffering from diarrhœa, and when that is suspended, leaves the hospital in as good health as prior to the attack;—such a case must be recorded as one of diarrhœa, but complicated with other disease. Hence, some phthisical patients find their way into the class of diarrhœa patients, and it has consequently been the practice to enumerate all who have had diarrhœa as a prominent symptom; but just as a [illegible] o of blood in the sputa is not called hæmoptysis, so one or two loose reference [illegible]ns are not classed as diarrhœa.

Imperfect as this Report necessarily is, not only in consequence of the inherent difficulties in the way of a complete system of registration, but still more because no similar documents have been published from which suggestions might have been drawn, it has yet been the endeavour of the medical registrar to render it as complete as the proposed plan would permit, and neither time nor labour has been spared to obtain the greatest possible accuracy in all the details.

It is to be hoped that the example set by St. George's Hospital in this matter may speedily be followed by other hospitals, and that this may form but the commencement of a series of similar documents, which cannot fail to be productive of good.

Cases admitted during the year 1853.						Admitted during three years.	
Nature of Disease.	Admitted.	Died.	Per centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per centage of mortality.
Fevers:							
Continued fever	108	15	13·88	23	8 [1]	374	12·04
Influenza	10	...	...	1	...	21	..
Asiatic cholera	1	1 [2]	100·0	...	...	1	100·0
Eruptive fevers:							
Measles	5	...	...	1	...	8	..
Scarlatina	5	2	40·0	2	2 [3]	29	17·3
Varioloid	4	...	...	1 [4]	...	10	..
Erysipelas	18	4	22·2	5 [5]	4 [6]	64	17·2
Intermittent fevers:							
Quotidian	4	...	...	2	...	13	..
Tertian	7	...	...	...	...	20	...
Quartan	1	...	...	...	...	5	...
Irregular	1	...	...	...	...	4	..
Rheumatism:							
Acute	52	3	5·77	16	3 [7]	165	3·64
Subacute and slight	88	...	...	21	...	303	...
Chronic	139	1	?	28	1 [8]	411	...
Gout, including rheumatic gout	28	1	?	4	1 [9]	69	...
Poisoning:							
Irritant	2	2 [10]	100·0	1	1	8	25·0
Narcotic	2	...	...	...	...	6	16·6
Syphilitic and gonorrhœal	9	...	...	8	...	29	.
Colica pictonum	8	...	...	7	...	25	...
Entozoa:							
Intestinal worms	3	...	...	...	...	17	..
Echinococcus hominis	...	...	...	...	...	2	100·0
Dropsy:							
Anasarca	100	39	39·0	100 [11]	39	297	33·3
Ascites	22	9	40·9	18 [12]	9	52	53·8

[1] Of the fatal cases,
1 was complicated with disease of heart.
1 „ „ phthisis.
1 „ „ disease of liver.
1 „ „ hæmorrhage from bowels.
2 „ „ peritonitis.
1 „ „ pleurisy.
1 „ „ pneumonia.

[2] Fatal in about five hours.

[3] Of the fatal cases,
1 was complicated with pyæmia.
1 „ „ scrofula.

[4] One case of varioloid eruption commenced in the hospital. Mode of introduction not known.

[5] Three cases of erysipelas commenced in the hospital.

[6] Of the fatal cases,
3 were complicated with disease of kidneys and dropsy.
1 „ „ phthisis.

[7] All the fatal cases suffered from pericarditis.
1 also complicated with endocarditis.
1 „ „ pleurisy.
1 „ „ disease of liver and subsequent ascites.

[8] The only death resulted from albuminuria and dropsy.

[9] The only death resulted from albuminuria and dropsy.

[10] The fatal cases were,
1 of poisoning with sulphuric acid.
1 „ „ caustic alkali, which was followed by ulceration of stomach.

[11] In 90 of these cases, organic disease of the kidneys, heart, or lungs was present. In the remaining 10, of which 2 proved fatal, the dropsy was dependent merely on anæmia and exhaustion.

[12] In 16, disease of the liver or peritoneum was clearly made out as the cause of the ascites. In the remaining cases (none fatal), its cause was not determined.

Nature of Disease:	Cases admitted during the year 1853. Admitted.	Died.	Per centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admitted during three years. Admissions.	Per centage of mortality.
Hæmorrhages:							
Epistaxis	5	...	...	3	...	11	9·1
Hæmoptysis	23	6	26·08	23 [1]	6 [2]	56	19·6
Hæmatemesis	4	2	50·0	3	2 [3]	18	11·1
Hæmaturia	6	...	...	5	...	22	4·5
Intestinal	11	2	18·18	6 [4]	2	24	20·8
Uterine	8 [5]	...	...	2	...	18	...
Purpura and scurvy:							
Purpura	6	4	66·6	5	4 [6]	18	27·8
Scurvy	3	...	...	2	...	3	...
Anæmia	66	...	...	28	...	171	1·1
Chlorosis	18 [7]	...	...	4	...	65	..
Cachæmia	9	4	44·4	9	4 [8]	27	33·3
Scrofula	12	4	33·3	11	4 [9]	25	16·0
Tubercular diseases:							
Phthisis	158	50	31·65	87	34	382	36·6
Tubercles in peritoneum	7	6	85·7	7	6 [10]	16	87·5
Tubercles in brain	4	4	100·0	4	4 [11]	8	100·0
Morbid growths:							
Hydatids	1	1 [12]	100·0	1	1	6	66·6
Encephaloid	10	8	80·0	7	7	20	65·0
Scirrhus	32	4	12·72	10	4	67	11·9
Colloid	...	...	...	...	...	1	100·0
Growths from bone	1	...	...	1	..	2	..
Hysteria	51	...	...	25	..	163	
Chorea	14	..	...	4	...	53	3·8
Delirium tremens	13	1	7·69	2	...	40	7·5
Diseases of brain and spinal chord:							
Cephalitis	10	8	80·0	7	7 [13]	28	82·1
Chronic disease	8	4	50·0	5	4	25	28·0
Apoplexy	4	1	25·0	4	1 [14]	15	60·0
Epilepsy	33	5 [15]	15·15	12	4	33	17·7

[1] Associated in 18 cases with phthisis.
" in 2 " disease of heart.
" in 3 no organic disease could be made out.

[2] In one case only the cause of death.

[3] In both cases the chief cause of death.

[4] Two occurred in fever, one of which was fatal. The other fatal case was one of those recorded under hæmatemesis.

[5] Besides 12 of menorrhagia.

[6] All the fatal cases were complicated with organic disease of viscera, especially disease of heart and kidneys.

[7] Besides 10 of amenorrhœa.

[8] The fatal cases were all examples of purulent contamination of the blood, 1 from abscess in the course of scarlatina, 3 from phlebitis.

[9] In only one case was scrofula the cause of death, under the form of large strumous abscess.

[10] Four cases had also tubercles in the lungs.

[11] All the cases admitted with cephalic inflammation. Three had also tubercles in the lungs.

[12] Cyst in arachnoid, causing epilepsy.

[13] Of the fatal cases, 7 occurred in patients of a tubercular diathesis. Of these, 4, as already stated, had tubercles in the brain.

[14] In the fatal case, there was disease both of the heart and kidneys.

[15] Of the fatal cases,
1 had a cyst in arachnoid.
1 very slight recent effusion of blood.
2 old disease of the kidney.
1 encephaloid disease of the liver.

Cases admitted during the year 1853.						Admitted during three years.	
Nature of Disease.	Admitted.	Died.	Per centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per centage of mortality.
Functional disturbance	49 [1]	...	...	[illegible]	...	114	.
Inflammation of cord	1	..	.	1 [2]	...	5	60·0
Paralysis:							
Hemiplegia	34	2	5·85	10	2 [3]	84	7·1
Paraplegia	20	.	..	6	...	60	10·0
Local paralysis	6	1 [4]	16·6	2	...	21	4·7
Neuralgia:							
Tic douloureux	3	...		...	...	[illegible]	...
Sciatica	14	..	...	5	..	[illegible]7	
Hemicrania	3	..	...	2	..	4	..
Angina	1	...	...	...	.	2	..
Diseases of the heart:							
Pericarditis	16	9	56·25	16 [5]	9	45	35·5
Endocarditis	6	3	50·0	6 [6]	3 [7]	38	7·9
Hypertrophy	16	13	81·25	16 [8]	13	75	62·7
Dilatation	15	11	73·3	15 [9]	11	41	46·3
Valvular disease	51	14	27·45	39 [10]	14	164	21·4
Diseases of arteries and veins:							
Aneurism	5	2	40·0	4	2	11	27·3
Phlebitis	8	3	37[illegible]	5	3 [11]	20	30·0
Diseases of respiratory organs:							
Laryngitis	10	5	50·0	6	5 [12]	20	40·0
Tracheitis	2	..	.	..	.	2	.
Bronchitis	150	19	12·6	84	17 [13]	371	13·7
Pneumonia	34	10	29·4	20 [14]	8	108	26·8
Pleurisy	60	16	26·6	34 [14]	15	148	27·0

[1] Seventeen cases presented more or less the character of insanity.

[2] Associated with paraplegia.

[3] In the fatal cases, hemiplegia was associated—
in 1 with disease of kidney and effusion of serum in the ventricles.
in 1 with chronic inflammation of brain.

[4] The fatal case presented no organic disease. Paralysis first attacked the muscles of the eyes, then the fauces, and lastly the muscles of respiration.

[5] Two had also endocarditis. Pericarditis was associated—
in 8 cases with acute rheumatism.
in 3 „ disease of kidney.
in 1 „ phthisis.
in 4 „ pleurisy.
Pleurisy also co-existed in 3 of the rheumatic cases, and 2 of those with disease of kidney.

[6] Endocarditis was associated—
in 4 cases with acute rheumatism.
in 2 „ pleurisy.

[7] Two of the fatal cases already enumerated under pericarditis.

[8] Twelve had disease of the kidney.
9 had also dropsy.

[9] Eleven had dropsy.
5 had also disease of the kidney.

[10] Nineteen had dropsy.

[11] All fatal from purulent contamination of blood.

[12] All the fatal cases were phthisical.

[13] Of the fatal cases,
10 were associated with disease of heart or kidneys.
4 „ „ emphysema.
2 „ „ pleurisy.
1 „ „ peritonitis.

[14] Among those enumerated as complicated cases, eight had both pleurisy and pneumonia.

Nature of disease.	Cases admitted during the year 1853. Admitted.	Died.	Per centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admitted during three years. Admissions.	Per centage of mortality.
Emphysema	13	6	46·2	13 [1]	6	32	31·2
Asthma	2	...	...	1	...	2	...
Pertussis	2	...	...	...	...	2	..
Pneumothorax	...	...	...	...	...	5	80·0
Diseases of mouth and pharynx:							
Quinsey	7	...	...	...	...	29	...
Enlarged tonsils	2	...	...	2	...	13	.
Ulceration	4	...	...	...	...	10	10·0
Mumps	[illegible]	...	...	1	...	8	...
Diseases of stomach and œsophagus:							
Dyspepsia	66	1 [2]	1·52	29	.	166	0·6
Ulceration	2 [3]	2	100·0	1	1 [4]	4	75·0
Diseases of intestinal canal.							
Obstruction	1	1	100·0	1	1 [5]	4	50·0
Constipation	48	...	..	20	...	154	
Diarrhœa	27	3	11·1	12	3 [6]	75	8·0
Dysentery	5	1	20·0	[illegible]	1	11	36·4
Tympanitis	4	...	.	8	.	10	.
Ulceration	3 [7]	2	66·6	3	2 [8]	8	87·5
Diseases of peritoneum:							
Acute peritonitis	19	7	36·8	11	6	63	39·1
Chronic peritonitis	17	8	47·1	13	8	36	50·0
Diseases of liver and gall bladder:							
Inflammation and congestion	5	2	40·0	4	1	17	23·5
Cirrhosis	19	15	78·9	18 [9]	15	37	72·9
Jaundice	18	6	33·3	12	6 [10]	49	24·5
Enlargement	9	2	22·2	5	1	27	22·2
Gall-stones	1	..	...	1 [11]	...	2	
Diseases of spleen							
Enlargement	4	..	...	3	...	9	22·2
Diseases of urinary organs:							
Nephritis	2	..	..	...	...	10	10·0

[1] Of the complicated cases,
12 were associated with bronchitis.
1 „ „ phthisis.

[2] The only fatal case was one of great dilatation of the stomach, without any evident organic lesion.

[3] No cases have been entered as ulceration, in which the fact was not ascertained by post-mortem examination.

[4] This case already enumerated as the result of poisoning with caustic alkali.

[5] The result of malignant disease.

[6] Of the fatal cases,
1 was associated with phthisis.
1 „ „ malignant disease and ulceration.
1 „ „ albuminuria.

[7] Those cases are not enumerated in which ulceration was a symptom of fever or of phthisis.

[8] Of the fatal cases,
1 was due to malignant disease.
1 „ chronic peritonitis.

[9] Seventeen of these had ascites.

[10] Of the fatal cases,
[illegible] had cirrhosis of liver.
2 had malignant disease of liver.
1 had disease of the heart.

[11] Accompanied by jaundice.

Nature of Disease.	Cases admitted during the year 1853.					Admitted during three years.	
	Admitted.	Died.	Per centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per centage of mortality.
Cystitis	5	..	...	2	...	17	5·8
Albuminuria	71	43	60·1	[illegible] [1]	43	228	50·5
Diabetes	5	1	20·0	3	1 [2]	13	15·4
Diseases of ovaries:							
Dropsy	7	3	42·8	2	2 [3]	16	25·0
Tumours	7	...	...	1	...	19	...
Diseases of uterus:							
Amenorrhœa	10 [4]	...	..	7	...	38	...
Menorrhagia	12 [5]	...	...	6	...	28	...
Leucorrhœa	[illegible]	..	...	9	..	37	
Tumours	4	...	...	2	..	8	12·5
Prolapsus	3	.	.	..	...	15	.
Congestion	2	...	.	..	...	5	
Diseases of bones and joints	6	...	..	5	...	30	20·0
Diseases of skin and cellular tissue.							
Erythema	8	..	.	6	...	39	
Urticaria and roseola	3	.	..	1	...	7	28·5
Lichen and prurigo	2	.	..	2	...	6	.
Squamous eruptions	9	...	.	5	.	30	...
Vesicular eruptions	19	..	..	9	..	51	...
Pustular eruptions	7			5	...	18	.
Pompholix and rupia	3	..	..	1	..	10	10·0
Cellular inflammation	3	1	33·3	2	1 [6]	18	55·5
Diseases of muscles	1	1	100·0	1	1 [7]	1	100·0
Anomalous and accidental cases	6	...	..	1	..	19	.

[1] Complications—
60 cases with anasarca.
28 „ disease of heart.
18 „ disease of lungs.
6 „ phthisis and scrofula.
9 „ disease of brain and paralysis.
13 „ diseases of other abdominal viscera
7 „ rheumatism and gout.
3 „ erysipelas.
1 „ diffuse cellular inflammation.
1 „ scarlatina.
2 „ purpura.
2 „ fever.
2 „ cancer.

[2] Death from phthisis.

[3] Complicated in both instances with chronic peritonitis and ascites.

[4] Besides eighteen of chlorosis.

[5] Besides eight of hæmorrhage from uterus.

[6] The fatal case was one of diffuse cellular inflammation, associated with dropsy and albuminuria.

[7] Abscess forming in centre of belly of rectus femoris muscle, followed by secondary pneumonia.